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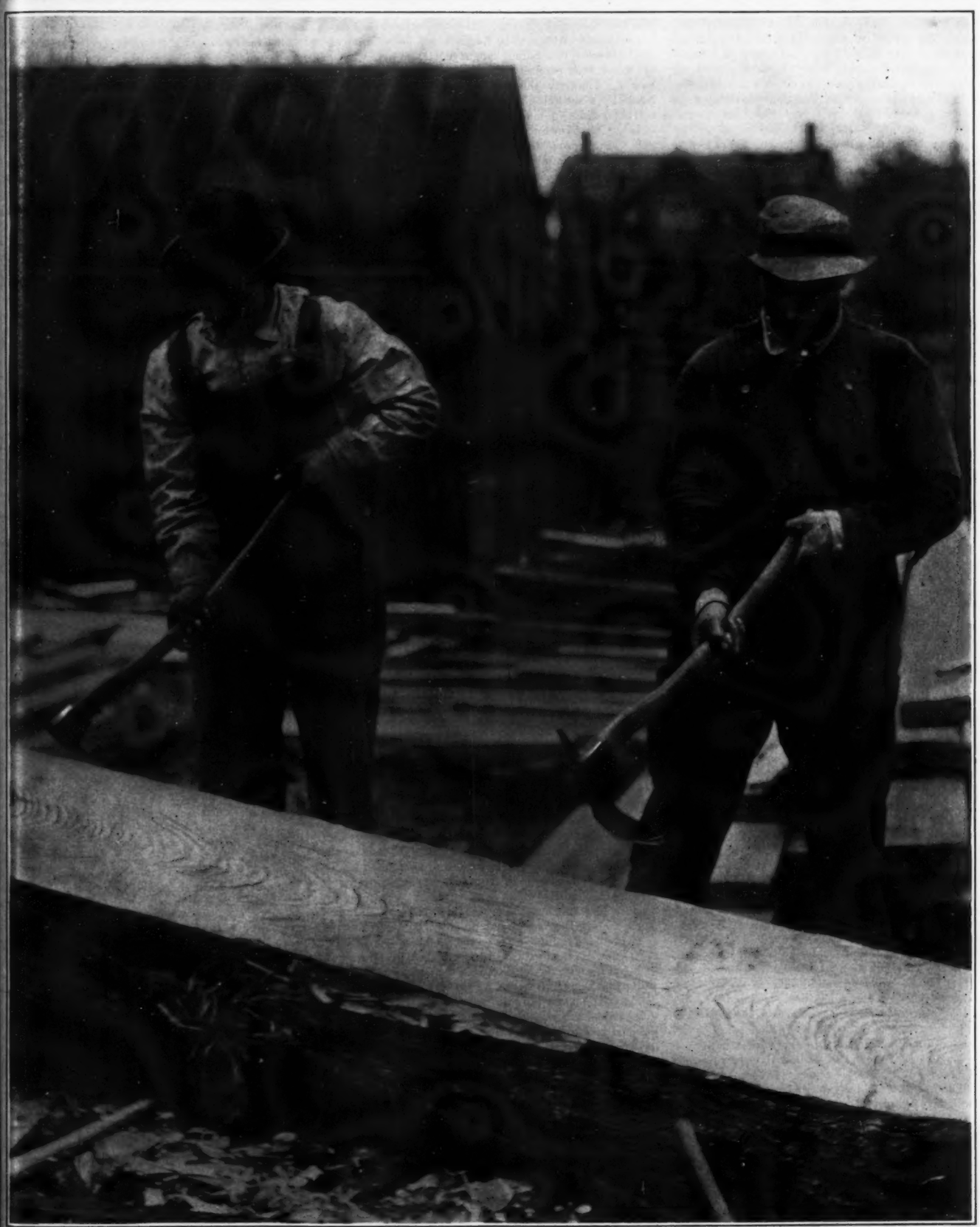

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Adzmen shaping a timber for a wooden ship

A REVIVAL OF WOODEN SHIPBUILDING [See page 376]

American Indian Languages

A Study In Method*

By Truman Michelson, Bureau of American Ethnology

At the very beginning of this subject it should be stated that there is no single type of speech which holds good for all American Indian languages. The statement that all American Indian languages are both polysynthetic and incorporative, so confidently affirmed by the older writers, is false. The number of American Indian languages that are either polysynthetic or incorporative, is extremely limited indeed. I do not know of a single feature that may be said to be characteristic of all American Indian languages. Even so, a combination of certain features is quite sufficient to determine whether any given language is an American Indian language or not. It is this which enables us to say without any hestitation that Chuckchee, Koryak, and Yukaghir (which are spoken in northeastern Asia) are Americanoid languages. If they were spoken in America we would call them American Indian languages. They do not belong genetically (as far is as known) to any American linguistic stocks, but nevertheless the totality of their features compels us to classify them with American Indian languages. To account for these facts we must assume either that these tribes are the descendants of the forefathers of American Indians who did not follow their brethren in the migration from Asia to America (for such a migration is firmly established by the facts of physical anthropology), or that there has been in comparatively recent times a migration backward from this continent to Asia. Inasmuch as the bulk of American Indian languages are spoken on this continent, from a purely linguistic point of view the latter hypothesis is the most probable. But the essential fact in any case will remain unchallenged, namely, that we have Americanoid languages spoken in northeastern Asia.

Let us now turn to the almost unparalleled number of linguistic stocks on this continent. We have an apparent anomaly as compared with most parts of the world. The point at issue is whether such a multiplicity of stocks is original or not. In the first place, in the study of American Indian languages we are at a decided disadvantage as compared with the study, say, of Indo-European languages. Suppose that modern English and modern Russian were the sole survivors of the entire stock. It would be impossible to prove absolutely that they were both genetically descended from a common ancestor, no matter what we might surmise. It is only because we have continuous written records of both covering several centuries, and have the aid of other related languages which have even earlier records, that we can absolutely prove this. For this reason it is clear that there always will be American Indian languages whose genetic connection we may suspect, but which we can not prove. It may be urged that we can actually see what has taken place in the development and differentiation of languages which have been historically transmitted, such as Indo-European languages, and that we should apply the principles derived from such a study to American Indian languages in determining the stocks. The methodical error in such a procedure lies in this, namely, that there are less than half a dozen different stocks in the entire world of which we have records, going back continuously for more than a thousand years. The percentage of the total stocks so transmitted is altogether too small to afford a firm and sure foundation for such a mode of action. If we could establish from a minute study of the dialects of some dozens of stocks that the kind of differentiation, etc., was on the whole of a similar nature in these stocks, we would be entirely justified in applying the principles derived from such a study to the determination of the limits of stocks in American Indian languages and other stocks as well. Unfortunately such a study has not been made, nor is there any prospect of it being done in the immediate future.

We have a similar difficulty in the reconstruction of parent languages of American Indian linguistic stocks. In the case of Indo-European languages we again can take advantage of principles derived from a study of the historical development of the separate members of the stock, and apply the results to the prehistoric period. We can not do this in the case of American Indian languages. The nearest approach to this would be a very minute study of the dialects of known stocks. In some cases there is no doubt that this would even largely counterbalance the difficulty spoken of. For example, most of the dialects of the Algonquin stock are so closely related that it can readily be ascertained in at least many cases what is archaic and what is secondary. Thus it is certain that the Fox *e* and *ĭ* vowels are more primitive than the Ojibwa *i* vowel, and that the terminal vowels preserved in Fox, Sauk, Kickapoo, Shawnee, and Peoria, but not appearing Ojibwa, Ottawa, Potawatomi, etc., are archaic.[1] Hence these features are to be ascribed to the Algonquian parent language. However, we can not know that precise quality of the prehistoric *e* and *i* vowels. Similarly the combination of a sibilant followed by a surd stop in Cree is more archaic than the correspondents in many of the related languages, and so is to be likewise ascribed to the parent language. (The actual proof that the Cree combination is more archaic is too complicated to be given here, as it would of necessity be altogether disproportionate to the length of this paper.) Naturally, in some instances, absolute proof would be wanting. Thus in certain cases Ojibwa *nd* corresponds to Cree and Menominee *ʻt*, Fox *t*. It can be easily shown that the Fox *t* in this case is in all probability unoriginal; and most philologists will assume that Ojibwa *nd* is more archaic than Cree and Menominee *ʻt*. But probability and assumption are not the same as proof.

A further obstacle to the reconstruction of the parent languages of American Indian stocks is our frequent inability to formulate phonetic laws in a manner as such is demanded by all modern Indo-European philologists. These contend that phonetic laws in themselves admit of no exceptions; and that wherever we find apparent exceptions, there is some extraneous reason, or reasons. The historical study of the individual Indo-European languages shows that analogy and the like have been potent factors in transforming them, and are fully as important as the actions of phonetic laws. For example: late Latin *potēbam* is not a phonetic transformation of Latin *poteram*, but is due to the influence of other imperfects in *bam* preceded by a long vowel. For this reason we are justified in extending the principles derived in this manner to the prehistoric period, to harmonize discrepancies among the historical languages which cannot be accounted for by phonetic laws. Thus the Italic languages have an ablative singular of *ā* stems in *ād* (retained in Oscan and early Latin; final *d* lost in classical Latin by phonetic law). The collective study of Indo-European languages shows conclusively that the ablative singular of *a* stems was the same in form as the genitive. Since the same study demonstrates that *o* stems in the Indo-European parent language had an ablative singular in *ōd* (preserved in early Latin; *d* lost phonetically in classical Latin), and that no other stems in the Indo-European parent language had a special case form for the ablative singular; and since we know that in historical Indo-European languages analogy has been a potent transforming factor, we have an entirely legitimate right to assume that the Italic languages developed an *ād* ablative singular for *ā* stems by the influence of the *ōd* ablative singular of *o* stems. In one sense the proof is not absolute, but it is as absolute as it is possible to give when dealing with prehistoric phenomena. It must be admitted that at times even Indo-European philology is at sea, and that purely subjective speculation may come into play. Happily these instances are rare. These remarks are inserted because although the facts are well-known to Indo-Europeanists, they are largely unknown to Americanists.

The bearing the above has on the problems of American linguistics is this: since American Indian languages have not been transmitted to us in the manner that Indo-European languages have, we do not know what has disturbed phonetic laws in many given cases, and for the methodical reasons outlined above, we are not justified in assuming that the same influences have been at work in American Indian languages as in Indo-European languages. Herein lies our difficulty in formulating phonetic laws that are entirely satisfactory to the Indo-Europeanist. For example: *n* becomes *c* in Fox before *ĭ*, which is a new morphological element; it remains if the *ĭ* is not such an element. There are some specific grammatical categories in which the law does not work. A study of several related dialects shows that this change also takes place in them, and hence must be very old. At the same time the apparent exceptions have not been explained. Whether they ever will be, is questionable. Yet an Americanist does not object to the formulation of the law as it works in practice. The Indo-Europeanist will object vigorously to such a formulation as it is, contrary to his accepted canons. If the canons of Indo-European philologists be accepted, it is quite evident Americanists can not reconstruct the parent languages of American Indian stocks in an entirely satisfactory manner. However, most Americanists are far more interested in observing actual phonetic correspondences and the like between the different dialects of linguistic stocks as they actually occur, than in speculations which from the nature of the case must rest upon rather slim foundations. It may also be noted in this connection that Indo-Europeanists have begun to interest themselves more with the linguistic problems of historical languages, and less with the remote Indo-European parent language.

Let us return again to the question of the multiplicity of American stocks. As stated above, this is today almost without parallel. However, it does not follow that this has always been the case. In Europe we know definitely that Etruscan has been wiped out; but we do not know how many distinct stocks were obliterated by the spread of Indo-European languages. It is entirely possible that many have been so obliterated. If they have, we have then a case quite analogous to the situation in America. But this is merely speculation. The problem may be approached from a different point of view. There is no reason to suppose that the migration from Asia was all from a single stock, in other words, that the differentiation has all been on American soil. Were that the case, in spite of the enormous lapse of time, surely we would be able to find at least one striking morphological trait common to all American Indian languages, for the morphology of a language is its most permanent feature.

Though, as intimated above, we have an apparent multiplicity of stocks which can not be reduced, nevertheless a number have resemblances to each other. An example is Siouan and Muskogean. The question resolves itself to this : Are these resemblances indicative of a common origin so remote that it is no longer possible absolutely to prove it, or are such similarities due to borrowings? To settle the question we must know what may be borrowed. That sounds may be borrowed across extremely divergent linguistic stocks is abundantly proved by the languages of the Northwest coast where we have the condition that languages whose morphology and vocabulary are distinct have practically the same phonetic elements. That vocabulary may be borrowed across linguistic stocks is too well-known to require illustration. That syntax may be borrowed across linguistic stocks is shown by the languages of Mexico where Spanish syntax has patently influenced that of American Indian languages. At this point we may ask a question, namely, can morphological features be borrowed? This is one of the most pressing problems of linguistic science awaiting solution. Unfortunately we have little material at hand definitely to prove or disprove it. Such as we have tends strongly to establish it. For example it is patent that the post-positions of Wishram (Chinookan stock) are due to the influence of Sahaptian, a distinct though contiguous stock. Similarly, classifiers are common to the Salishan, Wakashan, Chimmesyan, Koluschan, and Skittagetan stocks which are at the same time contiguous. One may suspect, indeed, that the first pair, and similarly the last pair, have differentiated from a common ancestor. Yet at the very best we would have a single striking morphological trait spread throughout three stocks which otherwise have nothing in common in either morphology or vocabulary, but only have resemblances in sounds. It is too great a strain on the imagination to believe that this is wholly the result of chance. If the accepted definition of stock is to remain, namely, a stock consisting of one or more languages all of whose sounds, morphology, syntax, and vocabulary genetically have descended from a single ancestor, we must admit at once that this trait which is held in common, is due to borrowing. For the differences in vocabulary and morphology are so enormous that it is inconceivable that the present differences are solely due to later differentiation. The vocabulary and morphology which cannot be explained at present are just as important as the extremely small percentage that can. There are a few other cases in which morphological borrowings between stocks is plausible, but they are entirely too few in number to warrant us at present in applying the principle broadcast. So we have to content ourselves in the meanwhile in pointing out structural resemblances between stocks, such as between

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[1] Amer. Anthrop N. S., 15: 470. 1913; this JOURNAL, 4: 403. 1914; Ann. Rep. Bbr. Amer. Ethnol., 28: 274. 1912.

Esquimoan and Algonquian, which resemble each other strikingly in their pronominal systems, and to a much less extent in the building up of verbal stems, in the hope that a careful and minute study of all the dialects of such stocks may enable us definitely to affirm whether such traits are due to early differentiation from a common remote ancestor or to comparatively recent borrowings.

It must be admitted that recently there has been a decided tendency among Americanists to consolidate such stocks as show only a moderate amount of common lexical and morphological resemblances. In some cases, such as Uto-Aztecan, it must be conceded that the burden of proof is now definitely on those who maintain that the two "stocks" are true distinct stocks, and not differentiations from a common ancestor. It is on the other hand equally certain that the genetic connection has not been established with absolute nicety. In the same manner Athapascan, Koluschan, and Skittagetan are almost certainly genetically related. Their morphological resemblances are so numerous and so special that it is incredible that they are due solely to borrowings, and not to genetic relationship. However, the amount of lexical material the three have in common is an exceedingly small percentage of their total vocabulary. The extremely large percentage of the unexplained lexical material forces us to admit that this has been derived from outside sources, and with our present definition of "stock," a purely genetic relationship between the three breaks down. After all, our difficulties all hinge on our definition of "stock," and the proofs necessary to show that one or more languages constitute such a "stock." Though the definition of "stock" given above, may be rigorously correct, the actual application of it would practically obliterate the total number of "stocks" in the world, and we should be worse off than ever. For in that sense, there are few, if any, languages which constitute a stock. In the writer's opinion philologists have taken over biologists' concepts without inquiring whether they are suitable to their own science. "Stock" must be redefined in a way that has some real meaning, and some term or terms invented to cover those larger groups which apparently are only remotely related, which may be related in sounds, morphology, syntax, and vocabulary, but not all combined as a unit. It goes without saying that the nature of the proofs then demanded will be in accordance with our definitions.

To revert to a point brought out above: If the morphology of languages can not be borrowed, with scrupulous accuracy we must assume an enormous number of distinct stocks at the very dawn of man, which certainly is not plausible. If on the other hand, sounds, morphology, syntax, and vocabulary have no innate connection (*vide supra*) then we would have a series of various borrowings instead of "stock" in its present sense. It may be well however, to state that no matter how "stock" is defined at present, it certainly is not used in that sense alone, but in a more loose way, almost according to the whim of the author.

The most decisive proof that two or more langauges belong to the same stock, used in a somewhat free sense, is numerous and detailed resemblances in their structures.

A large percentage of vocabulary held in common is a welcome additional proof. The most decisive proof that a single language constitutes a special stock is numerous unique morphological features. No amount of purely lexical resemblances between languages, no matter how far apart geographically, would prove that they belonged to a single stock. For, experience has shown us that vocabulary is very often borrowed in large amounts, and hence is not a good criterion. If the tribes were far apart geographically, that would not preclude the possibility that in prehistoric times they had been in contact, and at that time extensive borrowing had taken place. Another reason why vocabulary is not a good criterion is that the number of words in even distinct stocks that superficially resemble each other is really considerable. An example is Sanskrit (Indo-European stock) *áśan-*, Fox (Algonquian stock) *mas'*, both meaning "stone." Comparisons of vocabulary are only valuable when we know that the morphology of the languages compared are the same, or at least very similar. Otherwise we should not know whether we were dealing with comparable elements, even if the words in their totality resembled each other. For example, the comparison of the pronoun Avestan *čiš*, Greek τις, Latin *quis*, Oscan *pis*, all meaning "who," is entirely justifiable, because the structure of all four languages is fundamentally the same; and, which is also important, it has been shown that though these words apparently resemble each other only slightly, that as a matter of fact the correspondence of the various sounds forming these words is precisely what we should expect from our knowledge of comparative Indo-European phonology. Another example of a case where words of two different stocks superficially resemble each other is Sanskrit *naṣṭa-* "dead," Fox *ne'tōwe* "he kills."

It will be recalled that the combination of a sibilant followed by *t* appears as *'t* in Fox. As soon as we note that Sanskrit *naṣṭa-* is composed of the elements *naṣ* + *ṭa-* and Fox *ne'tōwe* of the elements *ne* + *'tō* + *we*, the comparison ceases to interest us. [The Fox word can, as I think, be reduced still further in analysis, but this only still further emphasizes the point at issue.] We should exercise the same prudence in comparing morphological elements. For example the verbal termination of Latin in the third person singular is *t* which superficially resembles Tsimshian *t*, to say nothing of similar terminations in various Algonquian dialects. Again Greek *μεν*, the verbal termination of the first person plural, has an entirely fortuitous resemblance to Ojibwa *min* in *ni—min*, *ki—min* of the independent mode.

Turning now to the classification of the languages belonging to single stocks,—it must be said that very little work has been done on this important topic in American linguistics. The three stocks of which we have the best knowledge in this respect are Salishan, Siouan, and Algonquian. To a certain extent the classification is arbitrary. We select a number of salient features and base our classification on it. In most cases we have overlappings which are indicative of more than one association. For example, Peoria fundamentally belongs with the Ojibwa division of Central Algonquian languages; at the same time there are certain traits which clearly prove that it has also had an association with the Sauk, Fox, Kickapoo group, also one with Cree; and there are some indications of contact with Delaware-Munsee. Nevertheless in spite of such shortcomings, we can make classifications which are entirely satisfactory even to the Indo-European philologist. The object of our classifications is to determine the prehistory of the tribes of any given stock. For example, the Abnaki dialects exhibit so many special traits in common with Shawnee, as well as Sauk, Fox, Kickapoo, that it is absolutely certain that in prehistoric times the tribes speaking these dialects have been in long and intimate contact. In the same way the Central Algonquian dialects are all so closely related, that it is evident that the place of diffusion must have been of limited area. The present geographical distribution of the tribes is insignificant in comparison with the evidence obtained by linguistic procedure. Nevertheless it has its value. It can not be accident that the most divergent Algonquian languages are spoken at the extreme western boundary of the stock.

This last brings us to the question of whether the differentiation of a stock into various dialects is due to evolution without external influence or whether it is due to linguistic shock. To this we reply that every case must be judged on its own merits. Peoria, though spoken today by only a handful of people, has been and is, exposed to far greater linguistic shock than Ojibwa which is spoken by several thousand persons; and, notwithstanding, is in many essential points a far more archaic language than Ojibwa. At the same time its diverse affinities indicate a series of shocks. Similarly, it can hardly be doubted that the divergent character of Aleutian (Esquimauan stock) is due to such shock. It should be candidly admitted that our studies in American linguistics have not yet reached a point where these problems can be answered in a thoroughly satisfactory manner.

It will be remembered that we are not in the position of the Indo-European philologist who can observe what has happened in the differentiation of dialects in a space of several centuries, and thereby draw legitimate inferences as to what happened in the prehistoric period. We also lack good time-measures; so we can not tell how long it has taken such and such a dialect to differentiate itself from its kindred. In this connection it should be stated that the Indian words cited by early travelers and missionaries are so badly recorded, that if the language from which the word is cited is still extant, ordinarily we can do far better with the words spoken today. The methodical error of using the analogy of Indo-European languages in solving the points at issue, has been shown above. Our guesses from such inferences might be very happy, but they would remain guesses, not proved facts. Our whole endeavor should be to place American linguistics on a rigid scientific basis, not upon a foundation of guesses. If I have spoken strongly on this point it is because some of the most eminent Americanists apparently desire to do the latter. They wish, without sufficient reserve, to consolidate stocks that apparently resemble each other, and are content to leave it to the next man to prove or disprove their cases. I do not deny that very likely they have made some extremely happy guesses, but I do deny that the *evidence thus far produced*, has been of such a nature as to compel acquiescence in all the results they claim to have obtained.

The opponents of the views here expressed will reply that the proofs demanded are more suitable to the so-called exact sciences than they are to philology; that if such a program be adhered to, we can not possibly hope to accomplish our task, as the languages are rapidly disappearing; that they are interested only in great facts, not minutiae; and that Indo-European philologists concern themselves largely with the latter simply because the great facts of Indo-European philology are known, and hence there is nothing else left for them to busy themselves with. To all of which the following rejoinder may be made: The proofs demanded may possibly be more rigid than those the Indo-Europeanists ordinarily demand, but it should be noted that every year the proofs demanded by the latter are becoming more and more rigorous, so that the disparity is after all being rapidly reduced, and in time doubtless the proofs demanded by philology will be fully comparable to those demanded by the exact sciences; that there is no advantage in erecting a magnificent edifice on such a weak foundation that it may topple over any minute; it is better to do even a little, and that portion well; and lastly, though it may be granted that Indo-Europeanists today are primarily concerned with minutiae, the discoveries of Grimm, Grassmann, Verner, Brugmann, Collitz, Schmidt, Ascoli, de Saussure—all of which were due to more rigorous methods—are of capital importance; and lastly it should be observed that centuries of quasi-scientific study of languages preceded the discovery of the genetic relationship of Indo-European languages, which of course will be counted as the great fact of Indo-European philology.

To change to a different topic. The study of American Indian languages will show that there is no such thing as superiority in language. They all possess the necessary machinery to express the most complicated ideas. The point is simply that what is grammar in one language may be vocabulary in another, and *vice versa*. For example there are no prepositions in Algonquian languages, but what we call prepositions are expressed by grammatical processes or special features in vocabulary. Thus "on" will be expressed by the locative case. "To come in" in Fox is *pīti*, "to go out" is *nōwi*. Nor is the language of primitive peoples indicative of low mentality. Thus Fox has but two fractions, one-half and one-quarter. It does not follow at all that the Fox Indians can not conceive of other fractions. As a matter of fact they can. The point is that ordinarily their life is such that there is no necessity for expressing them. I have tested this again and again by asking interpreters how to say, "Give me a third of that pie," and the like. In every case they were able without the slightest hesitation to render the idea, though not the precise words. I mention this simply because Gobineau apparently still has a goodly number of followers and admirers.

In conclusion, the study of American Indian languages is an extremely attractive field for students of general linguistics; and one that as yet is almost virgin soil. By their study the Indo-European specialist will find his scope vastly broadened, especially in the so-called philosophical bearings of his science. The American ethnologist who neglects their study, places himself wholly at the mercy of interpreters; or at the best must rely on the *ipse dixit* of his wiser colleagues. For these reasons their scientific study should be fostered in every possible way.

A Bromide Cleaner

EVERY year thousands of bromide prints are thrown away because they are over-printed or over-developed and look dull and heavy, while as many are sent out in the honest belief that they are as good as the negative will yield. Many of these in the first category could have been saved, and those in the second greatly improved by the use of the iodine-hypo reducer, which is not so well known as it deserves to be. One of its great advantages is that it is quite uniform in its action, that is to say that it will take a shade off the entire surface of the print without eating away detail in the high-lights and without staining or giving a granular texture. It is nothing more than a very dilute aqueous solution of iodine. If a drachm of iodine is dissolved in a couple of ounces of water, using sufficient iodide of potassium to effect solution, a dark brown tincture is obtained. To clear the prints, we put enough water in a dish to cover them, and add to it a few drops of the iodine solution, a very little will give a sherry color, and this is a suitable strength. The prints are immersed, a few at a time, until the paper turns a deep blue and the high-lights of the image show a pale blue. The print is then rinsed and transferred to an ordinary fixing bath, say, hypo 3 ounces to the pint. The blue is instantly discharged and the print gains in contrast and clearness. Washing for the same period as after fixing completes the process. With bromide paper at its present price it is well worth trying this simple method, especially for enlargements. —*British Journal of Photography.*

Detecting the Pretense of Deafness

Tests Adopted in France to Reveal the Tricks of Slackers

THE fears that the violent detonations of modern high explosives would result in a large number of cases of complete and permanent deafness has happily proved ill-founded, though there is a certain percentage of men who have become deaf as the result of actual lesions of the middle or the inner ear. Since, however, such detonations often result in temporary deafness, a strong temptation is offered to would-be malingeres on sick-leave to pretend that the affliction is more serious or more permanent than is really the case. To circumvent such shirkers new and ingenious tests have been devised by aurists in the military clinics. These are described in a late number of *La Nature* (Paris), by Dr. Robert Foy, Chief Surgeon of the Service for Reëducation of Audition and Speech in the Tenth District. After explaining that the object of the medical men is not the punishment of the effort at fraud, but the desire to induce the culprit to see the error of his ways and amend them, Dr. Foy says:

Fig. 1. Acoumetric induction (the patient lifts his hand when he begins to perceive the vibrations of the receiver)

To reveal their very human and temporary trickery, the classic methods of examining the auditory function are insufficient. These (phonic acoumetry, diapason, watch), are in fact based solely on the verbal response of the subject to divers auditory sensations to which he is subjected; in order to detect our "exaggerators" and "continuers" it is necessary to have tests based on exterior objective manifestations which are reflexes of the degree of audition.

There need be no fear of letting these be popularly known; it makes little difference whether the invalids are acquainted with them; they can but submit to their precision. And the very fact that they know of infallible means of detection will prevent them from exaggerating their deafness or persevering in pretense. We will pass rapidly in review: 1. The minor methods of detection, rapid processes which give merely probabilities or presumptions; 2. The main methods, recent processes of greater precision and yielding certitudes.

Processes of Presumption.—First, some of the old methods.

Distract the attention of the invalid; then suddenly drop a five-franc piece behind him; if he turns abruptly, he is able to hear. This test has little value, since here there come into play heavy vibrations of solids, transmitted by the floor to the skeleton of the individual.

The test of the brush is more subtle. Stroke the back of the invalid on one side with a brush, on the other with the palm of the hand. If he distinguishes between the two, he can hear, for the distinction can be made only by the auditory perception of the scratching of the bristles on the cloth.

Tests of Voluntary Reflexes.—1. In the course of the otological examination, with the ear not being examined plugged, suddenly command the invalid, using demi-voice, to close his eyes or to cough. Frequently he will obey though he is making a pretense of bilateral deafness, for these are irresistible reflexes; this test proves that the invalid can hear the spoken voice at a distance of about 50 cm. (about twenty inches).

2. Place the invalid at a distance of three meters (about ten feet), from the examiner with his back turned, and the ear not under examination plugged. Then say in a normal voice: "As soon as you can hear me lift your hand." Then walk rapidly toward him repeating the vowel *A A A*, etc., without cessation. In many cases the pretender will raise his hand as soon as the examiner touches him; hence he was able to hear an order given in a normal tone at a distance of 10 feet.

Test by Words at Graduated Distances.—Bandage the eyes of the patient and place three or four assistants at various distances from the ear to be tested, the other ear being stopped; then let each assistant in turn utter rapidly any series of words; surprise and confusion will tend to make a pretender repeat words uttered by assistants stationed beyond the limit of perception declared by him.

Test by the Lombard Deafener.—If we cause the receivers of a telephone to vibrate in the ears of a normal individual reading aloud, we note a marked elevation of the voice; he seeks thus to dominate the noise of the receivers which deafens him and keeps him from hearing himself talk. The man pretending to be bilaterally deaf will raise his voice like a normal person. The genuinely deaf man as a rule will not raise his voice; but there are so many exceptions to this rule that the test can not be accepted as having an absolute value. All these tests are applicable only to very pronounced bilateral deafness. Here are others for unilateral deafness:

Tests by Acoustic Tubes.—Two assistants read, in whispers, two different texts into separate funnels, each connected respectively with an ear of the patient by a rubber tube. The "mono-deaf" man will repeat what is read into his good ear; the simulator of unilateral deafness can repeat nothing; for hearing both the texts will confuse him.

The Test of Simulated Obturation.—If we speak to a man genuinely deaf in one ear in a normal voice on the deaf side with the sound ear stopped by a perforated plug, he will repeat our words because he hears them through the sound ear. But the pretender will repeat nothing, since he believes that the ear is really stopped up.

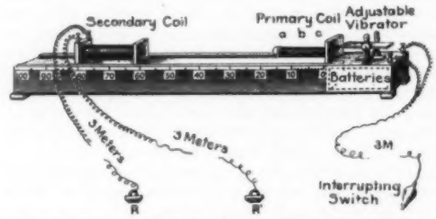

Fig. 2. Acoumetric inductor permitting the variation of intensity of sound in the telephone receivers, R, R'

PROCESSES OF CERTITUDE. (Personal Methods.)

Instrumentation.—The same apparatus answers for all these tests. It is the Faradaic apparatus of Gaiffe (Lombard type), which we have slightly modified in its accessories (Fig. 2). It is composed of a coil receiving the current by induction from a fixed induction coil which is placed within it and from which it can be removed by sliding on a carriage one meter long.

In the interior of the box two dry batteries give a steady current until they are used up. A vibrator regulated to its maximum speed permits us to obtain vibrations of a constant value and near to do_2 (medium) in two telephone receivers very equally balanced and connected with the secondary coil. A push button connected with the apparatus by a long conducting wire, permits us to interrupt and reëstablish the current, and consequently the vibrations reëstablish from a distance.

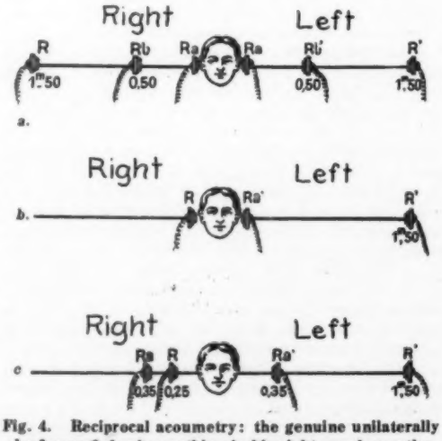

Fig. 4. Reciprocal acoumetry: the genuine unilaterally deaf person, having nothing in his right ear, hears the left receiver at a distance of one meter

I. *Acoumetric Induction.* (Fig. 1). In this test one of the receivers is applied to the ear being examined, the other serves as a control to the operator. The patient is seated as far as possible from the apparatus with his back to it. The secondary coil is placed at the end of the slide at one meter (39 inches), the normal distance of perception, then brought slowly towards the primary coil until the patient perceives an auditory sensation (threshold of audition). If the test is repeated two or three times, trickery on the part of the patient is easily detected by the difference in his responses.

This apparatus permits us, moreover, to appreciate the value of an audition very exactly and to follow, day by day, the progress of an otological treatment.

Fig. 3. Palpebral reflex. A winking of the eyelid is suddenly produced in the listener when the current is unexpectedly turned on

II. *Palpebral telephonic reflex.* (Fig. 3).—With the secondary coil barely in contact with the primary coil, we apply one of the telephone receivers to the ear of a normal subject; we surprise his audition unawares by making the receiver vibrate electrically. This sudden sonorous impression will cause the reflex action of an abrupt winking of the eyes.

In cases of moderate deafness it is necessary to insert half the primary coil within the secondary coil to obtain this reflex. In cases of severe deafness the reflex is not produced until the primary coil is entirely covered. In cases of total deafness there is no reflex.

This test completes the preceding one, in the sense that the patient who pretends to hear nothing in the course of the progress of the secondary coil on the carriage ought not to have the cochleo-palpebral reflex. In the contrary case deceit is evident.

III. *Reciprocal Acoumetry.* (Fig. 4).—This test rests upon the following principle: "Every sound perceived by one ear prevents on the opposite side the perception of sound of equal intensity but at a greater distance."

The patient is seated with his back to the apparatus; a mask over his face prevents him from seeing the operator's different maneuvers. The secondary coil is barely in contact with the primary coil, and remains in this position throughout the test. We establish the current. The tests can be interpreted by the diagrams.

1. In a *normal* subject (Fig. 4a), each ear being examined separately, each of the receivers is perceived separately, for example, at 1.5 meters, the one at *R* and the other at *R'*. If we place *R* in contact with the right ear *Ra*, the opposite ear can no longer perceive *R'* except at the contact *Ra'*. The perception of *R* at the contact covers the perception of *R'*. In the same way, placing *R* for example at 0.5 meters (*Rb*), it will be necessary to place *R'* at about 0.5 meters from the left ear (*Rb'*) in order for it to be perceived.

2. *In case of complete unilateral deafness,* (Fig. 4b), this is genuine if *R'* continues to be constantly perceived at 1.5 meters whether *R* is, or is not, in contact with the right ear. Since the right ear perceives nothing, the perceptions of the left ear are not modified.

The deafness is simulated (Fig. 4), when, *R* being in contact, we are obliged to put *R'* at the contact (*Ra'*), for it to be perceived by the subject.

3. In case of *partial unilateral deafness.* (Fig. 4c).—Suppose for example that the patient does not perceive *R* at the right except at 25 centimeters. This partial deafness is genuine if, *R* being placed beyond the auditory limits indicated by the patient, for example, at 35 centimeters, the left receiver, *R'*, continues to be perceived at the customary distance of 1.5 meters.

This partial deafness is simulated (Fig. 4c), if, *R* being placed as above at 35cm. at *Ra*, the receiver *R'* must be

placed at 35 cm. from the left ear to be perceived (*Ra'*). In fact it is certain that the patient hears at the right since the perception of the left receiver is influenced. By trials we can determine the true distance of perception of the receiver *R*. This test applies equally to *partial bilateral deafness.*

IV. *Process of the writing machine.*—If in a few rare instances a doubt persists in a case where complete bilateral deafness is claimed here is the ultimate test. The operator simulates in his turn, by accepting the deafness as genuine. The patient is trained in lip-reading for a month. At the end of this time he is summoned.

This is the scene: the patient is in front of us at a distance of 3 meters. Three meters to our left, let us say, is a typist, apparently very much absorbed in her machine. For several minutes we talk to our patient *in a whisper;* anxious to obtain his discharge, he responds marvelously, much too well. At an agreed signal the typist begins to strike her machine with zeal, its noise drowning our whispers admirably; the patient can hear nothing, he understands nothing, he answers nothing; he is a simulator; I can attest that he is able to hear a whisper at a distance of 3 meters; he can give no excuse. If he is really deaf he will feel no embarrassment for the plot will have passed over his head. After three months of work he would know no more, for it is impossible to learn lip-reading if one is able to hear. And if he refuses to try to learn it is a confirmation of his simulation. A really deaf person is only too happy to make even considerable effort to again be in touch with his kind.

These are the methods by which we may obtain a very precise appreciation of the functional value of an individual's audition. But, as my master, Lermoyez, recently made the remark, it is necessary to make a distinction in these evaluations between the physiologic audition whose limits are well beyond our real needs (spoken voice 50 meters, whispered voice 15 meters), and practical useful audition, of much more restrained limits (spoken voice 5 meters, whispered voice 50 centimeters). And it is of the latter alone, from the military point of view, that we need to determine the limits.

My personal experience enables me to conclude that a subject hearing the secondary coil at 50 centimeters (acoumetric induction), having a cochleopalpebral reflex, with the secondary coil half covered by the induction coil, possesses a very sufficient, practical, useful audition.

Flicker Phenomena

By Lindley Pyle, Associate Professor of Physics
Washington University, St. Louis

CLOSE the switch of an electric fan while peeping between its blades at some well lighted object. The intermittent flashes of light reaching the eye from the illuminated object give rise to a pronounced flicker until the fan acquires, or exceeds, a certain critical speed. Above this critical speed a high frequency flicker is still present, but the eye is unable to detect it. A light sensation on the retina of the eye persists a short time after the light itself is cut off. This "persistence of vision" explains the invisibility of high frequency flicker.

Thus, suppose a fire-fly, at rest, were to send out, say, one hundred flashes per second; the eye would interpret this as a steady glow. This hallucination would disappear if the fire-fly were to fly across the line of sight at, let us say, a speed of one hundred inches per second, meanwhile emitting light as above mentioned. The effect would then appear as beads of light strung along at intervals of one inch, the retinal images not being superposed as in the stationary case. A photograph of the line of flight taken with one second exposure would show one hundred spots of light.

Fig. 1. Swinging rod illuminated by alternating current arc. Photographed by stationary camera

Figure 1 is a photograph of a single swing of a nickel-plated rod swinging from one end and illuminated solely by an arc light fed by an alternating current. Of course, the rod does not emit light of its own as does the fire-fly; it reflects light whose source is the arc lamp. The photograph shows clearly that the arc yields intermittent illumination. The time of swing having been observed, careful measurements on the original negative indicated an emission of 120 pulsations of light per second, well above the flicker frequency that the normal eye can detect under stationary conditions. The arc was attached to what is known as a sixty-cycle current supply, in which the current flow is reversed in direction 120 times per second. This means that the current falls to zero value 120 times per second, with a corresponding frequency in the heating and lighting effects of the arc.

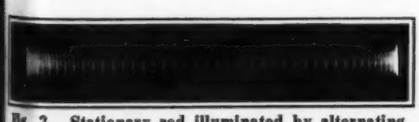

Fig. 2. Stationary rod illuminated by alternating current arc. Photographed with moving camera

Figure 2 differs from Fig. 1 only in that the rod hangs stationary and the camera is rotated around a vertical axis during a time exposure. The image of the illuminated rod moves across the photographic plate and makes a permanent record of its varying brightness.

Fig. 3. Stationary Mazda lamp photographed with moving camera
Upper: Lamp on direct current circuit
Lower: Lamp on alternating current circuit

Figure 3 is a photograph of a stationary Mazda incandescent lamp obtained by the moving camera method. The upper record shows the lamp burning on so-called *direct* current of unvarying strength, and indicates an unvarying intensity of luminosity of the lamp filament; the lower record shows the lamp burning on sixty-cycle alternating current, and yields unmistakable evidence of a markedly varying brightness of filament.

This interesting alternating current phenomenon may be viewed directly, under arc or Mazda lamp illumination, by shaking rapidly to and fro any small, bright object. Multiple images will be seen so recorded in our photographs. Of course the illumination does not fall intermittently to *zero*, since the electrically heated body does not have time to cool below red heat.

The illumination falling upon a moving picture screen is intermittent. Many times per second the screen is absolutely black, the dark interval being of very short duration. Since the retinal sensation outlasts a flash of light the dark interval is bridged and the eye is unaware of the trick played upon it. However, the trick is exposed if the observer waves a piece of white paper to and fro in the light reflected from the screen. The paper assumes the appearance of Fig. 1 and indicates that there are moments when no light is reflected from the screen. Further, when there comes an intense patch of light upon the screen, if the observing eyes be rolled rapidly from side to side, or up and down, the patch of light appears drawn out into a band broken across by narrow black streaks. This latter phenomenon is analagous to Fig. 2, obtained with a moving camera; the image of the patch moves across the retina impressing a record of its varying brightness.

The Eskimo Year

THE Labrador Eskimo, like their congeners in other sections, divide the year into seasons corresponding to the appearance of game or other natural conditions. These divisions do not correspond exactly with our monthly divisions, but are near enough for purposes of comparison. There is no attempt to equalize the lunar with the sidereal year, and the divisions, as their names indicate, are governed by the conditions of climate and the appearance of game.

On the east (Atlantic) Labrador coast, the following months are named:

"Ice-forming month," December.
"Coldest month for frost," January.
"Ground cracked by frost," February.
"The month of the young Jar seal (*ne'lceq*)," March.
"The month of the young bearded seal (*teyel-ut*)," April.
"Month of fawning," May.
"The month of the young Ranger seal," June.

According to my informant, the summer months were bunched into one season. He said there were many kinds of game then, and no necessity for distinguishing the season of any particular one.

From Ungava the following divisions, which distinguish the summer months, were obtained.

The months were said to be the same as given above until the month of June. (The young of the Ranger or Freshwater seal, from which the month of June takes its name on the east coast, is not found in Ungava.) Beginning, then, with June, we have:

"Egg-month," June.
"Mosquito-month," July.
"Berry-month," August.
"Fading-month," *i. e.*, the month when the leaves and mosses fade in color, September.
"The month when ice forms around the shore."
"Inland month," *i. e.*, the month that they go into the interior for deer.—*From Geological Survey, Canada, Anthropological Series, No. 14. By E. W. Hawkes.*

A Motor Car Railway Mowing Machine

THE accompanying illustration shows a construction of a novel oil motor car railway mowing machine which does $7 worth of work for 30 cents, that is, cuts heavy weeds and grass on both sides of the track at the rate of a mile every twenty minutes. This machine stops expensive scythe work on railroad grades, for it takes a section crew of four men with scythes a day to a cut a mile, at a cost of about $7.

This oil mowing car cuts twenty-five miles a day, at a cost of less than $7 for wages, gasoline and oil, and one machine covers 500 miles of track in three to four weeks; and as this is done twice a year one can figure the saving on 1,000 miles. If only half this mileage needed cutting, the scythe work costing only $3.50 per mile, there would be a saving of over $1,500 a year. Traveling three miles per hour, it mows a six-foot swath on both edges of the track at once. Either cutter bar can be tilted up or down at any angle, to follow the lay of the ground, which may vary on opposite sides of the track, and either one can be quickly and easily raised to avoid obstructions, or stopped and started without raising.

It is stated that with the cutter bars up and out of gear, used as a motor car, it makes fifteen miles per hour. It pulls itself on and off the track at road crossings, as the engine is free from the car. It starts without pushing and the two speed transmission develops great pulling power. This mowing car also has been shown to be just the thing for hauling extra gangs to work and moving materials, and three men handle it easily.

In a test this motor car, carrying an average load of five men, made thirty miles per gallon of gasoline used. These motor cars start without pushing, run equally well forward or backward, reverse instantly without stopping to change cams, and satisfactorily handle greater loads. An auxiliary by-pass and ball valve enables the car engine to run without missing on one tenth to one fifth of the gasoline required by throttled engines when running slowly, as must often be done. The engine develops four horse-power and weighs 540 pounds. The car hauls ten tons up any grade.

A Modern Commercial Volcano

IN a large power plant recently built there were installed five boilers, each of a normal rating of 1,140 horse-power. The furnace in each of these boilers is 24 feet wide and 17½ feet deep, and each furnace is fed by two mechanical stokers, which, under ordinary conditions put in about 3,800 pounds of coal per hour. They are, however, capable of feeding 30,000 pounds per hour. When feeding 28,000 of coal per hour the boilers are operating at 500 per cent capacity, making it a veritable mechanical volcano of fire and steam. The usual bed of coal is two feet thick, which means that there are at all times 23 tons of green coal and coal in the process of combustion. An advantage of this great mass of coal is that in case of a sudden demand for more power, there is a sufficiently great body of fire to enable the production of steam to be increased very quickly.

The Foundry Today*

A Survey of Apparatus and Methods For Iron Casting

By Benj. D. Fuller[1]

THE foundry has been undergoing a change more marked during the 15 years just passed than for many years previous. Conservatism and adherence to old-time methods had been the rule, and it has proved no small task to uproot some of the old ideas. The engineer has had much to do with the change, while the metallurgist and the analytical chemist have also played an important part. Time was—and not so long ago—when all iron founders made mixtures simply by fracture judging wholly by appearance, and many still do so. So expert did some of the old masters become that their product was indeed most perfect. Yet methods are such to-day as to practically eliminate the guesswork for appearances are as deceiving in metallurgy as they are elsewhere.

I well remember when the chill cast pig of foundry iron first made its appearance on the market what a decided objection there was to its closed-grained fracture. Experience taught that the closing of the grain due to casting in a chill would disappear upon remelting and that, in this case, it was entirely wrong to judge by fracture.

Formerly the mixing was shrouded in more or less mystery. The "old man" was looked upon with awe and reverence on account of his knowledge of the subject. Now the up-to-date foreman makes his mix in the office. He has the analysis of each car in stock and knows that for a certain class of castings the silicon, sulfur, phosphorus, etc., should be within certain specified limits; he knows the effect of each metalloid entering into the mix; he knows the difference between coke iron and charcoal iron, character of the iron from northern ore and from southern, and how to obtain the desired results.

As to the cupola itself and the method of melting there has been but little change. The design is practically the same as of yore. The interior of the cupola, as you are probably well aware, is brick lined, somewhat the shape of the outside of a milk bottle, with hinged doors on them, an opening for the extracting of the melted iron being placed on the side near the bottom. Above the bottom from 24 to 36 inches the shell is encircled by the tuyere box or wind jacket much as a ring of round or oblong pipe is run around the milk bottle, with a number of connections between the pipe and the interior. Just below the tuyere line and opposite the tap hole, at a higher elevation, is placed the slag-hole through which the melted slag which rises to the top of the bath is floated off.

Formerly the idea was generally held that the proper way to introduce the air was to form the tuyere as the nozzle of a hose shooting the air with velocity to the center of the coke charge. This idea has given way to introducing the flow in volume with not so much stress upon velocity. Tuyeres now generally extend the complete circle of the brick lining and are of ample area to introduce the flow easily. In this connection we now measure the flow of air and not the pressure. The pressure gage or water column simply indicated the pressure built up in the wind box and did not tell the volume entering the cupola. A meter has been devised and is now used which indicates the flow of air quite accurately, no attention being given to the pressure.

The charging of the cupola consists of closing the bottom doors, putting in a sand bottom on which is built the fire, with bed charge of coke extending from sand bottom to approximately 24 to 30 inches above the tuyeres; on this a charge of iron, next a charge of coke and so on. Formerly the practice was to have the charging platform and upper door through which the charging was done about eight to ten feet above the tuyere. Now most platforms and doors are placed from sixteen to twenty or more feet above the tuyeres, which means much heavier burdens are carried in the cupolas. This calls for a good quality of coke, particularly as to structure, as a coke of soft structure would pack down to such an extent under the heavy burden as to deter the melting and handicap the operation. Good coke, and let me emphasize this point, is most necessary in the production of good castings. A coke should be approximately 88 to 90 per cent carbon, not over one per cent sulfur and under 12 per cent ash. To-day, in the rush of things, we are getting much that is inferior and it is handicapping production in the foundries to a great extent. A high sulfur and low carbon coke will produce a sluggishly melted iron in which the gases and impurities will remain, not having the opportunity to "boil out" or escape as they will in the hot, fluid, well-melted iron. Again, a high sulfur coke will impart much of its sulfur to the iron in the melting, resulting in a hard, inferior casting.

Many foundrymen seem obsessed with the idea that above everything they should have a high melting ratio, sacrificing quality of the melted iron to a saving of coke in the cupola. They say with much pride that they melt at twelve to one, meaning an average of 12 pounds of iron to one pound of coke. Iron can be melted at this ratio, but in most cases it is not sufficiently hot to give good results, particularly in light castings. Eight to one is fair melting. Of course the higher the amount melted the better the ratio from the fact that the bed charge is generally about four times the amount used between charges above the bed, and the hotter the furnace becomes the less the amount of fuel necessary.

A process of reheating and refining iron electrically gives promise of being valuable and I would recommend to engineers this field of endeavor. Iron direct from the ore can be reheated, held at the desired heat and treated at will for the desired result. This is also being tried in connection with the air furnace as well as in the electrical.

Another field of promise in connection with the cupolas is the use of warm blast. It will no doubt be an improvement to heat the air before delivering it to the furnace. This is done in connection with the blast furnace, but not with the cupola.

In years past it was considered quite a feat to be able to operate a cupola continually for a run of ten hours, but this has become common practice in many places. Use of limestone or other suitable flux and proper attention to keeping slag-hole open in order that slag may be drained well, other things being satisfactory, permits successful operation for the entire day without trouble. A normal eight-hour melt for a cupola lined to 56 inches inside is 100,000 pounds. This may seem rather slow, but when castings on account of character of iron are demanded uniformly throughout the heat at 2,600 degrees, it is fair work for a cupola of size named and will supply approximately 150 molders all day on light work.

Taking care of slag-hole for an all-day melt will ordinarily be found troublesome, as the constant blast and flow of slag at the high temperature will melt and run away all but the most refractory material. A well-burned sleeve-brick answers well for this use. The slag flowing all day is a troublesome thing to care for, especially in hot weather. If a track be laid to run under the spout, a car can be provided on which is carried a large cast-iron box well tapered inside and washed with fire-clay to receive the running slag; the box to be, say, 6 x 5 x 4 feet deep and so tapered inside that when the slag cools and shrinks it will easily free itself when box is overturned, it can be cared for nicely. About two such boxes will suffice for a day's run.

Where two or more cupolas are grouped, blast pipes should be so arranged and connected to the different blowers as to permit the use of any blower in connection with any cupola by manipulating the slides. The most advantageous place for blowers is on a mezzanine floor underneath the charging floor.

Many arrangements of trackage, weighing devices, charging cars, etc., are being tried with more or less success and generally effecting an economy.

Overhead stock coke bins, into which the coke is discharged from elevators, where the men in charging can obtain the coke by opening a chute, which deposits it at the door of the cupola, are labor-savers with the advantage of an unobstructed floor.

The lining about the melting zone will require daily attention in the way of patching, a good grade of plastic fire clay being necessary for this use. Firebrick embedded in this clay is generally used, but for all-day service I consider a better material to be mica schist, a mica-bearing rock mined in New Jersey. This is being used in more than one Cleveland foundry to-day. However, the high freight rate mitigates against its use, the average car costing in freight alone about $130.00 delivered in Cleveland.

Semi-steel, as it is called, is now a direct product of the gray iron foundry. It is simply a gray iron charge to which is added an amount of mild steel scrap, say from 5 per cent to 50 per cent, added to which is a small per cent of manganese, its success depending largely upon the care used in charging and melting. If well handled in the cupola the product will be strong and machinable, but if poorly melted the product will be practically useless. Auto and motor cylinders generally contain from 10 to 20 per cent steel.

Sand with the following qualities is an important factor in foundry work: Resistance to burning when in contact with the hot metal; sufficient bond to adhere well when rammed or packed, but at the same time will not pack so tight as to prevent the flow or passage of gases which form when the molten metal enters the mold. These gases must escape through the sand or they will cause agitation of the metal and spoil the casting. Large, heavy work requires a coarse grain sand. The finer and lighter the work the finer the grain should be. Good molding sand contains from 80 to 90 per cent silica and some alumina. In steel casting work a sand higher in silica is necessary.

Until a few years ago all molding sand for gray iron work was mined and shipped direct from the field to the foundry, but to-day most shippers put the sand through a milling or screening process, many using both, hence a more uniform product is procured. Ohio and New York are large producers. In Ohio, the better grade fine sands come from near the lake and the heavy sands from the southern and central parts. The light sand veins are found directly under the top soil and run from 18 inches to 4 feet in thickness, while the heavy sand is often found in deeper veins from 8 to 12 feet in thickness.

One of the new developments in the iron foundry material is a sand made from ground and pulverized rock. The simple machinery generally used in this work is the grinding pan through which the rock or sand is passed before it goes over piano-wire screens to the car or stock pile.

Machinery is rapidly finding its way into the foundry. Among the more progressive are sand conveying, mold conveying, combination sand and mold conveying, all of which are meeting with favor. Sand cutting and mixing machines, independent of the conveying and carrying systems, are also being much used.

Some of the advantages of a good sand conveying installation are the mechanical mixing and tempering of the sand on which prepared material is delivered to the hand of the molder, the molding machine being placed directly under the sand chutes down which the tempered sand falls. The mold is cast almost as soon as it is made ready, the casting shaken out and the sand and casting soon removed leaving the floor practically clear most of the time. The molder has but a short distance to carry the mold from machine to casting point, saving steps and resulting in greater production. An average of six times the output from a given space is obtained; that is to say, six times as much floor space would be necessary to obtain the same production without the equipment.

In one installation the molding machines are arranged six feet apart along either side of a bay. The bay is 30 feet wide, each machine having a space 6 x 12 feet, leaving a six-foot aisle down the center. Underneath the floor and down the center the full length of the bay, approximately 250 feet, is placed a steel pan conveyor. Pans 12 inches in length linked together and moved by a 10-horse-power motor pass over sprockets at either end. On the floor down the center of the aisle and directly over this pan conveyor is placed a series of gratings upon which the molds are dumped, the casting remaining upon the grating while the sand falls through to the conveyor and is carried away to a revolving riddle at the end of the basement. Passing through this riddle the sand falls to a rubber belt which delivers it to a bucket elevator. The riddle through which it has just passed also removes the rough tailings. The bucket elevator carries the hot sand up 70 feet to a second revolving riddle; passing through it falls to another belt which delivers it to a disintegrator built on the pattern of a squirrel-cage wheel, the sand being received on the inside of the wheel and thrown out between the bars. Just before passing into this wheel the sand is sprayed with water. After passing the wheel it falls into a 45-ton tank where it pauses on its journey and becomes tempered and cooled somewhat. From this tank it is fed out on belts right and left, and carried back to the molder. An ingenious feeder has been devised to deliver a constant supply from the bottom of the tank

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to the belts. The tank tapers in toward the bottom until the opening is about fifteen inches wide by eight feet long. Under this opening is arranged a series of iron slats fastened on one side and moved reciprocally from the other giving a cutting motion which slices the sand onto the belt in a regular stream, the amount of which depends upon the size of opening between the bars and the speed at which they are moved.

Of the mold conveying type of equipment there are three worthy of mention as being successful. The one, such as adopted by the Westinghouse Air Brake Co., the Ford Motor Co., of Detroit, and others, consists of a train of cars or linked platforms moving on an elliptical track, the molders, ranged outside and part way around, place the open molds on the cars. Next cores are set by other men and farther on the molds are closed, clamped and cast. Moving on they are shaken out, the castings taken away, the sand passing on to be tempered by another set of conveying machinery and finding its way back to the molder. The flasks are put back on the cars and removed at proper stations. With the second type the platforms are suspended on a moving trolley, the operations otherwise being much the same as with the car system.

A third and effective contrivance which appeals by its simplicity is the merry-go-round, which is a ring platform supported from a central column and resembling a May-pole, the platform attached to the ends of the ribbons. Molds are placed on the ring as it passes round and round, the other operations being as above described.

Still another system which has its advantages requires a basement underneath the molding floor. The sand from each set of molding machines falls through a grating at the rear of the floor and is tempered below, passing forward during the operation, and is then lifted by a small bucket conveyor back to its own molding machine, each set of men controlling their own lift as the sand is needed by throwing a handy switch. This calls for a number of individual lifts, but has the advantage of keeping the individual sand heaps separated which, of course, permits the use of different grades of sand suitable for different classes of work.

The molding machine has done more than anything else to awaken an interest in advanced foundry ideas. There are many types from the plain squeezer machine where the sand is pressed into the flask either by hand power applied at the end of a lever or by compressed air, to the powerful jar machine capable of handling many tons of weight. In the latter type the mold is rammed by the lifting and dropping of a platen or table which supports the flask full of sand. The ramming is accomplished by jolting. Large machines of this type are generally served by a traveling crane and grab bucket for filling the flask. The power used is generally compressed air at eighty to one hundred pounds. Motor driven machines of this type are coming more into favor.

Another machine of a rather novel type carries the sand up in buckets and drops it into the flask on the floor from an elevation of twelve to twenty feet, the ramming being accomplished by gravity. Still another type is being perfected in which the sand is delivered to a conical-shaped container, at the proper moment an air pressure of 120 pounds is introduced to the container blowing the sand through holes in the bottom of the container into the mold with great force. This machine is doing remarkable work in the making of cores, but it is not as yet perfected for mold making.

In the core room as in the molding room machinery is fast finding favor in the form of sand mixers, conveyors, elevators, core machines, advanced type of baking ovens, etc., all of which call for ingenuity in designing core boxes, forming the proper sand mixtures, etc. It may be of interest to know that into the mixture used in the foundry enters such materials as flour, molasses, oil, glue, rosin, coal, coke, plumbago, soapstone, pitch and other numerous ingredients.

The cleaning department of the foundry has received much study in the way of equipment. In the cleaning of large castings the advancement has not been so marked as in the small work. Compressed air tools are generally used in the form of hammers, drills, wheels, etc. A novel method has been used in at least one plant where the casting is placed in a basin or reservoir and a stream of water under pressure turned on the cores washing the sand out. After the casting is cleaned the water is drained off and the sand removed from the basin. In another installation a large platform upon which the casting is placed is jarred up and down, shaking the burned cores out of the casting.

For small work the cleaning mill or revolving barrel is used. Into this barrel the castings are packed and among them is dumped an amount of cast iron jacks or bars. The dust and dirt is removed from the mill by an exhaust system, the dirt being caught in a cleaning chamber where it is separated from the air and passed out a chute below to cars for removal.

In the sand blast room the castings are blasted by a stream of sand of large grain, the sand and dirt passing down through the floor, which is a grating, is lifted by air to separators where the good sand is recovered, the dust passing on to the dust rooms and the good sand back to the blasting machine. The sand blast mill is similar to the ordinary mill except that streams of sand under pressure do the cleaning, the exhaust handling the sand and dirt as in the room equipment. The sand blast revolving table is a comparatively recent addition to the cleaning department. This machine is a table from six to eight feet in diameter which revolves at about three revolutions per minute, one half passing through a covered compartment where the castings are played upon by two or more streams of sand, the man outside placing the castings on the table, turning them over and removing those that are cleaned. Where production is large the rapid and systematic handling of castings is essential. They should always go forward from the mills to the grinder, chipper, packer and shipper, never back-tracking.

A tried and approved arrangement is to have cleaning mills and blast apparatus placed accessibly, castings deposited at mills and rooms by telfer or traveler, cleaned and loaded into the boxes or other suitable conveyance, delivered from mills to barrels by telfer to tables between double rows of grinders, the grinder after grinding depositing upon another table in front of wheels and arranged along behind rows of chippers, the chipper lifting the casting and putting on the finishing touches passes it forward to a traveling steel belt conveyor which carries it forward to the packer and shipper.

The proper lighting, heating and ventilating of shops, method of molding, securing and casting large and small work, the pattern shop and its tools, pattern making, foundry economy as to materials, foundry, accounting and kindred subjects which have a live interest to the foundryman, I will not attempt to include in this talk for fear of becoming tiresome, but one instance in pattern construction and method of molding a piece will be cited to illustrate wherein the pattern has to do with simplifying the molding operation.

Supposing we were to make a casting shaped as an ordinary oil barrel. The pattern would, in general practice, be separated through the center or divided into a couple of tubs. Place one tub down, place the drag half of flask (or lower part of mold box) around this inverted tub, ram in sand, roll over, place upper half of pattern on lower half of flask, ram up, lift the upper half, withdraw both parts of pattern and after finishing close mold.

A much simpler construction is to build a center column or pipe with loose cover on either end, the outer part of pattern to be built up as staves attached to the inner column of the pattern by bolts. In this shape the center pattern is rammed in, drag and center column drawn out first, then staves pick in. This not only simplifies the operation, but produces a better casting.

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Ferro-Concrete Vessels

THE fact that the building of ferro-concrete vessels has been commenced on a fairly large scale, first in Norway and now also in Sweden and Denmark, has naturally centered a considerable amount of technical interest on the question. In Sweden it has been discussed at some length, both in the technical press and at meetings of engineers.

One of the points which has been insisted upon by opponents of the material is the inability of concrete to resist the influence of salt water, however excellent it admittedly is in fresh water. The danger arises from the lime liberated, in the process of hardening, entering into combination with the sulphuric acid and magnesium salts of the salt water. At the instance of the German Concrete Union, the Prussian Department for Public Works, and other authorities, some extensive tests were commenced at the island of Sylt in the North Sea, and these tests have not yet been terminated. Concrete blocks were laid on the shore, fully exposed to the chemical and mechanical influences of the sea. The blocks were moulded in two mixtures, A and C, the cement of the former containing about sixty-five per cent of lime, and that of the latter only sixty-one per cent to sixty-two per cent. But the latter was richer in alumina and iron, ten per cent to twelve per cent, against six per cent to eight per cent in A. The concrete blocks were so placed on the shore that at high water they were entirely submerged, and at low water only partly so. After as little as two to three years the C blocks, even such as were moulded in mixtures 1:2, showed fine cracks, and after three to four years the surfaces had all suffered much. The A blocks, in a rich mixture, stood the test better, and in mixtures 1:2 they were unaffected after four to five years; with poorer mixtures, 1:4, the edges had suffered, and with still poorer mixtures were much affected after as little as one to two years.

Recent experiments in the United States have shown that an addition of five per cent of finely ground clay to a mixture 1:3:6 rendered it more resistant, also more so than the same mixture with 10 per cent of lime added. On the whole, however, all the test blocks suffered more or less.

In spite of all this there are numerous cases in which concrete has withstood the influence of salt, even very salt, water quite satisfactorily; it is in almost universal use for harbor works, etc. The current explanation of the unfavorable effect of salt water upon concrete is that the sulfate of magnesium in the water combines with the lime in the concrete, forming sulfate of calcium, which is of larger volume and therefore has a bursting effect upon the concrete. There are, however, many other kinds of cement, and especially cement of the so-called puzzolane type, not only free from a surplus of lime, but with a surplus of silicic acid, and thus able to bind free lime. The addition of such kinds of cement and other substances containing silicic acid has, therefore, for a series of years been used as a protection against the undesirable effects of salt water.

In the reports for 1915 from the Gross Lichterfelde West Material Testing Institution some exhaustive laboratory experiments as to the influence of different solutions upon concrete mixtures of 1:3 and 1:6 were dealt with. The concrete was, as a rule, exposed to the effect of the solutions during a period of six months, a comparatively short time, but, on the other hand, the solutions were strong, as much as 10 per cent, or entirely saturated. Portland cement, with 63.23 per cent of lime, was used. The following solutions were employed at the tests: (1) Chloride of sodium, up to 100 grammes per liter; (2) sulfate of sodium, up to 100 grammes per liter; (3) sulfate of calcium, saturated; (4) sulfate of magnesium, up to 100 grammes per liter; (5) chloride of magnesium, up to 100 grammes per liter; (6) chloride of calcium, up to 100 grammes per liter.

The 1:6 concrete was all more or less affected, but all the 1:3 concrete tests showed that no harm had been done to the concrete, except in one case, in which the block had been kept in a closed exsiccator during the hardening, and had been submerged in sulfate of calcium solution. The use of too much fine sand in the mixture is undoubtedly injurious. This was shown very plainly in *Le Cement* last year. The unsatisfactory result of the American tests already referred to, may to a great extent be attributed to the very fine sand which was used for the mixtures.

It is clearly evident that concrete can only be affected by sea water to the extent the water penetrates it. If the concrete is sufficiently dense, nothing but the surface is exposed to any risk, and the making of dense concrete is no longer a novelty. It goes without saying that more especially in concrete vessels every possible care is taken in this respect.

Protective measures in the construction of concrete vessels may be summed up as follows: (1) The use of rich concrete; (2) The use of cement with but little free lime, gypsum and alumina; (3) the addition of puzzolane to fix the free lime; (4) the use of coarse sand; (5) the use of dense concrete; (6) surface treatment. Should the cement boat-building industry develop, there should not be much trouble in guarding against decay if full use be made of our present knowledge.

In conclusion, the advocates of concrete as ship-building material urge that concrete in any case is more resistant than steel. The fact remains that reinforced concrete vessels are in existence and are being built, although it is premature at present to express any definite opinion as to their durability and strength.—*Engineering.*

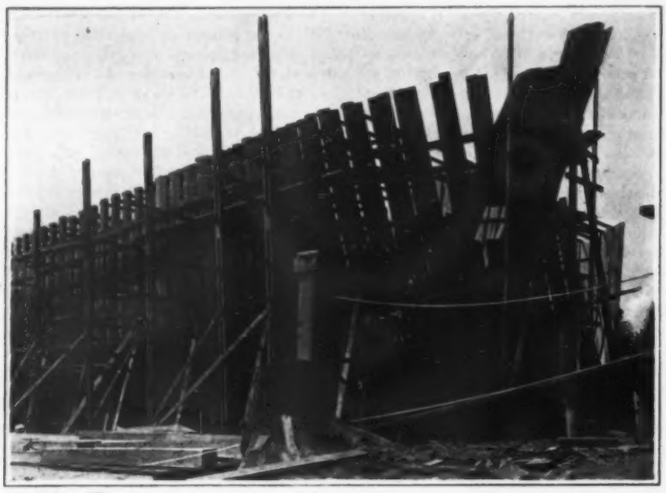

Photo by Brown & Dawson

Photo by Brown & Dawson

Two views illustrating the structure of wooden ships. At the left is shown details of the frame construction at the bow, from the outside; at the right, the interior, looking toward the stern

The Revival of Wooden Shipbuilding

An Ancient Industry Rehabilitated by the War

THE necessity for ocean commerce carriers, to make up the shortage caused by the great number of vessels of all nations destroyed by German submarines, has had the unexpected result of reviving the almost obsolete industry of wooden shipbuilding. On the Atlantic coast of North America most of the old establishments that were famous during a notable period of our history long since abandoned their efforts to compete with the iron and steel ship, and the few small yards that still continue in operation confine their efforts to turning out small coasting schooners, tug boats and lighters, for which there is a comparatively steady, although diminishing demand. On the Pacific coast, however, the business of building wooden ships has continued quite active for many years, although most of the vessels turned out have been of moderate size, and mostly designed for the rough work of lumber carrying.

As the war progressed, and the combined decrease in the number of vessels and the vast increase in material to be transported led to fabulous increases in freight rates, not only was every description of craft that would float requisitioned, but every ship yard in the country was overwhelmed with orders. Old yards where wooden ships were built years ago, but long since abandoned, were resurrected, and new yards have been organized every week along our entire coast line on both oceans, from Maine to Texas, and from Puget Sound to San Francisco; and now the United States Government has taken a hand, and proposes to have constructed a great fleet of wooden ships, in an effort to neutralize the destruction by U-boats.

Although steel has long since supplanted wood for vessels of all kinds, both steam and sail, the people of this country are still familiar with wooden craft, and, besides the actual necessities of the occasion, there is a romantic impression attached to the wooden ship that appeals to the popular fancy, and the revival of wooden shipbuilding is looked on in many quarters with favor, notwithstanding their evident inferiority in many essential features.

The art of building craft of wood is as old as history, and it is curious to note that many of the structural methods employed by the Greeks nearly 400 years before the Christian Era are still followed; thus we find the keel, the floor timbers of the frames, over which is placed the keelson, the curved stem, secured to the keel by the apron, and the thick wales, for strengthening the structure, all in the timber-built ship of today and in practically the same form as in the Triremes of Athens, ages ago. Moreover, in many of her parts the timbers are held together by wooden pins, after the custom of centuries. This does not mean that the art of shipbuilding has not improved, but that in some things the old builders had already devised the best methods of doing the work.

In many respects a wooden ship is a patchwork that is decidedly unsatisfactory as compared with steel construction, and although it has served its purpose well in the past, and is still useful in a limited degree, it cannot be regarded as a desirable thing to perpetuate, on account of the limitations of the material. Not to go minutely into the details of construction, a brief sketch of the building of a ship of wood will give an idea of its character. Upon a suitable foundation the keel of heavy timbers is laid down in the longest lengths available; but as it is impossible to get a timber of sufficient length for a vessel of 3,000 tons, which is about what the Government is proposing, it is necessary to "scarf" several pieces together, which is an elemental source of weakness. At the bow the stem is set up, a deep timber extending from the keel to the upper deck, and with the grain running as near as possible parallel with the desired curve. As we cannot get a piece of timber with a natural crook at the lower end, to round into the keel, it is necessary to insert a filling piece at this point, and the double

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Caulking the seams between the planks

joint here is bridged over by a curved piece of timber called an apron, which is bolted on the inside to the stem and keel. The stern post, which is a straight vertical timber, is easily attached. Across the keel are laid the "floor timbers" of the frame in pairs, and over these is placed the keelson, a heavy timber similar to the keel. Iron bolts are driven through the three parts, and headed over at each end to clinch the structure together into as nearly a homogeneous mass as possible. Usually, in a vessel of any considerable size, a number of timbers are worked to form the keelson, making a solid mass of material several feet deep, in order to furnish vertical strength and stiffness to the bottom of the vessel. The frames are next built up of three, four or more pairs of timbers on each side, arranged so that the joints between the ends of two successive pieces of one member of the frame alternate with the joints of the other member, and the two members of each frame are secured strongly together by iron bolts driven through and clinched. In place of these bolts it was formerly customary, and the method has not yet been abandoned, to fasten the parts of the frame, or ribs, together with tough wooden pins, or treenails, which were driven through holes bored for the purpose; the ends of these pins being split and expanded by wedges of soft wood, which swell with moisture, acquire a strong hold on the timbers. For securing the planking these same treenails are largely used, and they are one of the principal links that connect ancient and modern methods of shipbuilding.

For sheathing the vessel the longest planks obtainable are used, and the joints at the ends of the planks are distributed as far apart as possible; moreover, at some points extra thick planks are used, termed wales, and corresponding to them, on the inside of the frames, similar timbers, or stringers, are placed and treenails or bolts, are driven through all. An inner ceiling of heavy planks is also worked the entire length of the vessel. The framing of the decks is of heavy timber, to add to the strength of the structure; and to stiffen it, and to assist in preventing the vessel from twisting, strong knees are bolted in the angles between the deck timbers and the sides of the ship, both horizontally and vertically. These knees are natural crooks, cut at the junction of a large root or branch with the trunk of the tree.

To give an idea of the timbers required here are some dimensions of a recent design for an auxiliary schooner 225 feet long on the keel, and 260 over all. The keel is 20 x 20 inches; the stem 20 inches thick and 30 inches deep; stern post 20 x 24. The frames are double, spaced 30 inches from center to center, and each timber is 26 inches deep by 12 inches wide at the keel, 20 inches deep at the lower turn of the bilge, 17 at the upper turn, 12 inches at the main deck and 9 inches at the rail. The main keelson is 20 x 20, and the rider keelsons the same size. Three sister keelsons 20 x 20 are fitted on each side of the keelson.

The first strake of planking next the keel is 9 inches thick, the next is 7 inches thick; bottom planking 5 inches thick; wales 6¾ thick and top side planking 6 inches. At the turn of the bilge there are five strakes of ceiling on the inside, 14 x 14 inches and three strakes 12 x 12. Above the bilge the ceiling is all 10 inches thick. All of these immense timbers are piled up, layer on layer, in an endeavor to make the hull stiff enough to hold its shape—an endeavor never wholly successful, as, in a heavy sea all of the thousands of joints will work and permit leakage. In the better class of ships it is also customary to give additional strength to the hull by a series of heavy iron straps laid diagonally over the frames at short distances apart, and extending from the deck to the floor timbers.

From the above outline it will be appreciated what a patchwork a wooden vessel is, and how difficult it is to make it strong enough to hold its shape, and hold together, when, loaded with many tons of cargo, it is first

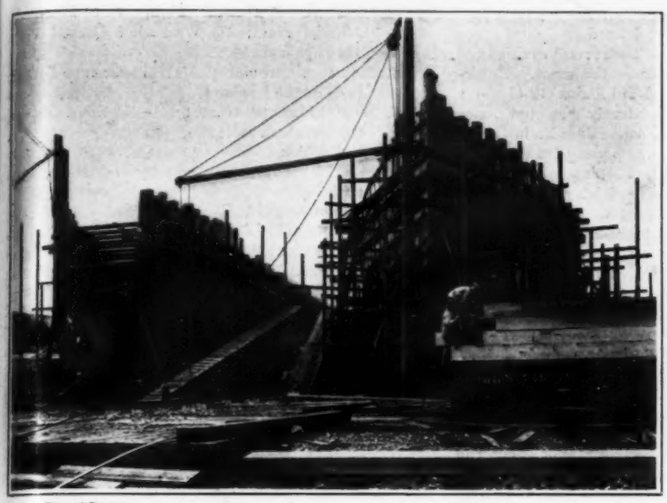

Photo by Brown & Dawson

Bow views of two vessels on the stocks

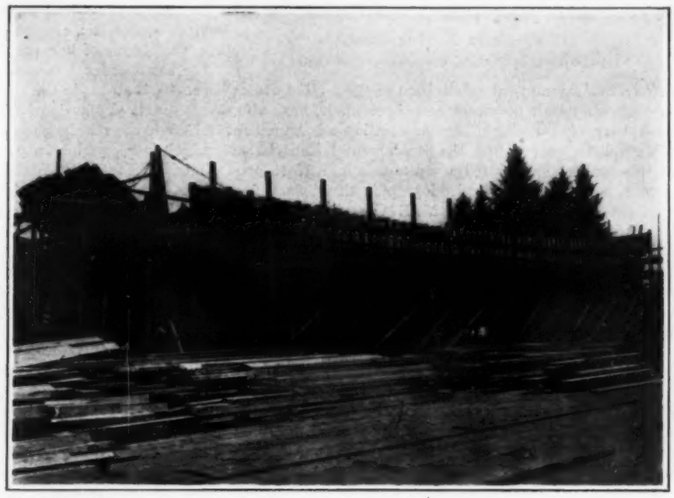

Photo by Brown & Dawson

A general view of a wooden vessel, from the stern

suspended by its ends between two big waves, and then lifted by a wave at its middle, and all the while it is being subjected to a complicated series of lateral strains. These difficulties, which are insurmountable when wood is the material used, are satisfactorily met by steel, for it is easy to bend and shape steel for the various parts; fewer parts are required, as a single piece of steel can be fashioned to perform a function that necessitates a number of separate pieces of timber, and when the steel parts are properly riveted together the resulting combination is practically as strong as a single piece.

Much has been said of the simplicity of a wooden ship, and that little skilled labor is required, but this is far from the fact, and a competent authority, who has had experience in building in both wood and steel, has stated that actually more skilled workmen are necessary for the construction of wooden ships. Of course modern machinery has replaced much of the hand labor that was formerly necessary, but there are still many operations where experience and special training are necessary.

As much special material is required for a wooden ship the old time builders had men constantly cruising the timber districts searching for desirable timber, and when suitable trees were discovered the builder sent his own men into the woods to fell the selected trees, and to divide them into sections that could be used to best advantages. Then the pieces were hewn to shape on the spot, which saved the transportation of useless material. The rough work of hewing was done with the ordinary axe, but the finishing was by the broad axe and adz. The broad axe, which is seldom seen now, had the shape of the fat, rounded hatchet, but the blade was eight or ten inches long, and the edge was chisel shaped, that is flat on one side, and with the bevel on the other. The handle was curved to one side, to enable the workman to work close to a log, without hitting it with his hands. These big tools were kept at a razor-like sharpness, and the skilled broadaxe man worked with such accuracy that he could split a chalk line; and he worked out crooked frame timbers with wonderful precision. For some portions of the work, where the broadaxe could not be used, the adz was employed, and the illustration on the front page of this issue shows two men working with this tool. An expert adz man prided himself on his ability to hew a plank down to a thickness of less than a quarter of an inch, and have it smooth and of equal thickness throughout.

By systematizing the construction of the framing it is now possible, by the use of circular and band saws, to produce by machinery many of the curved frame timbers that formerly had to be wrought by hand; still there is much work about a timber ship that must be done by the skilled hewer.

We read of old ships sixty and one hundred years old, and still sound, but these were almost entirely built of well-seasoned oak, a material that has become too scarce in suitable sizes to cut any great figure in the extensive plans now being formulated. To take its place recourse has been had to the softer, and far less durable woods, such as southern pine and the Douglas fir of the Pacific coast. These woods are not only much less durable than oak, but are not so strong, and consequently much more material must be used to secure the necessary strength. Moreover, dry and seasoned material cannot be procured, and the timber for the proposed fleets is still uncut in the forests. This feature is being belittled by those interested in the proposed fleets, but in the days when wooden shipbuilding was at its prime it was considered necessary, when good work was to be produced, that not only should the timber be well dried and seasoned, but the ships should be built under cover to keep them dry while under construction. The desirability of this procedure will be recognized by everyone conversant with the handling of timber, as it is well known how quickly green, wet timber deteriorates and rots when put into any structure on land; and the same reasons prevail with greater force in a ship.

In former days 300 feet length was considered excessive for a wooden ship, on account of the impossibility of making so large a vessel sufficiently strong; but it is now proposed to build them 350 feet long. To do this a New York naval architect has proposed improved methods of construction that appear to be very efficient; but they involve some features that result in serious inconvenience. For instance, it is proposed to extend the heavy keelson up to the upper deck, making a solid timber bulkhead the entire length of the vessel, which divides it into two separate compartments; and to convert this into an immense I beam by constructing flanges at each deck by working several heavy timbers on each side of this central bulkhead. This construction, with ingenious methods in other parts, should add very considerably to the strength of these big ships, but it necessitates a double set of hatches for the two divisions of the hold, and the space occupied by all of this material greatly diminishes the cargo space.

A timber ship will not carry as much cargo, size for size, as a steel vessel, and it will not carry it in nearly as good condition as it cannot be kept so dry. Moreover, the cost of upkeep of a timber vessel is much greater than one of steel, and it requires more attention to keep in operation. In view of the positive shortage of the necessary skilled labor there are serious doubts if a 5,000-ton vessel can be built of wood materially quicker than one of steel; and as they must be driven by power, and the ability to produce engines, either steam or internal combustion, would control the time within which these ships could be brought into service, it is open to doubt if anything can be gained by resorting to wood construction instead of steel.

England has requisitioned the entire product of her steel shipbuilding yards, and the vessels thus obtained will be of inestimable value after the war, and for years to come. In the American yards there are under construction thousands of tons of shipping, largely for foreign owners, and, with the future in view, these owners are forbidden by their Governments to sell their ships to go under another flag. This leaves us to our own resources in the future, and there is no good reason why we should not follow the example of our neighbors, and requisition the product of our steel yards for our own use. If, in the future, Congress continues its policy of forcing American owned ships to sail under foreign flags, this matter can be easily adjusted; but in the meantime we will be in a position to meet present and future emergencies, and will not be left with a big fleet of obsolete and practically worthless vessels with which to compete with other countries. At present unknown interests, possibly foreign, perhaps lumber owners, are promoting the plans for building wooden ships; on the other hand the cry is raised that the steel interests are opposing wooden ships for their own advantage. If this is the case they are working in the interests of the public as well, and criticisms of their promises of assistance in our present emergency are not well founded. Fortunately the able official in charge of the building of our emergency fleet appreciates the situation and is directing his efforts accordingly.

Protecting Merchant Vessels Against Submarines

EXPERIENCE accumulated during the past year as to the particular features of construction which are calculated to enable a ship to remain afloat after being subjected to torpedo or gunfire attack, and attention has recently been given to the possibility of incorporating in a standard design of merchant vessel some features of construction which would give a greater measure of protection from war risks. Professor T. B. Abell, to whom the technical questions involved have been submitted by shipowners, has pointed out that while protection against gunfire can be effected by fitting thick side plating or its equivalent above the water line in conjunction with special watertight subdivision in the neighborhood of the water line, extra weight of structure must thereby be entailed, and that if the protection is to be made effective the reduction of cargo-carrying capacity will be appreciable. Protection against torpedo attack is obtained in warships chiefly by watertight subdivision in conjunction with special steel bulkheads. Longitudinal sub-division, while practicable in a warship at great cost, is impracticable in an ordinary cargo vessel, and it is considered doubtful whether transverse water tight sub-division would enable a cargo ship to resist torpedo attack. It is essential that the ship should remain afloat with any two of these compartments open to the sea, which would mean that cargo holds must be shortened and the freeboard to bulkhead deck increased. The conclusion reached by those who, like Professor Abell, have given the subject special attention is that while the watertight sub-division of merchant vessels can be greatly improved to protect them against torpedo attack, considerations of design, economy of construction, carrying and earning power make it preferable to arm the ship. This is the course which will probably be adopted in the new tonnage now building.—*Engineering Supplement of the London Times.*

Hydraulic Power in Spain

THE use of hydraulic power in Spain for electric plants and for various mills has increased considerably within a recent period, this being mainly due to foreign capital. Late estimates for the total amount of power in actual use, give 5,000,000 horse-power in round numbers. There are now in operation 110 hydraulic plants in what can be called the important sizes, namely, upwards of 280,000 horse-power, but besides this there are a great number of smaller plants. Besides the electric stations there are a great number of mills in operation such as paper mills, flour mills, sawmills and others which use the water directly. In the Catalonia district the textile industries alone make use of 200,000 horse-power, of which, about one-half is hydraulic power. The total amount of water power from the Pyrenees is some 1,135,000 horse-power. Other figures relate to the amount of power that could be furnished by the large rivers of Spain, for instance the Ebro and its affluents could supply no less than 1,300,000 horse-power, and the Duero nearly 1,000,000, while the Quadalquivir and the Tagus can be called on for 750,000 horse-power each. The total amount for these and the other large streams is estimated at 5,000,000 horse-power.

Physiographic Subdivision of the United States*

By Nevin M. Fenneman
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VARIOUS attempts at subdivision of the United States into physiographic provinces have been made, beginning with that of Powell.[1] The Association of American Geographers, recognising the fundamental importance of this problem, appointed a committee in 1915 to prepare a suitable map of physiographic divisions. The committee consists of Messrs. M. R. Campbell and F. E. Matthes of the U. S. Geological Survey and Professors Eliot Blackwelder, D. W. Johnson, and Nevin M. Fenneman (chairman). The map herewith presented and the accompanying table of divisions constitute the report of that committee. The same map on a larger scale (120 miles to the inch) will be found in Volume VI of the *Annals of the Association of American Geographers*, accompanying a paper by the writer on the Physiographic Divisions of the United States. In that paper are gven the nature of the boundary lines and those characteristics of the several units which are believed to justify their recognition as such. Though the above-named committee is not directly responsible for the statements there made, many of them represent the results of the committee's conferences. The paper as a whole is believed to represent fairly well the views of the committee, though in form the greater part of it is a revision of a former publication.[2]

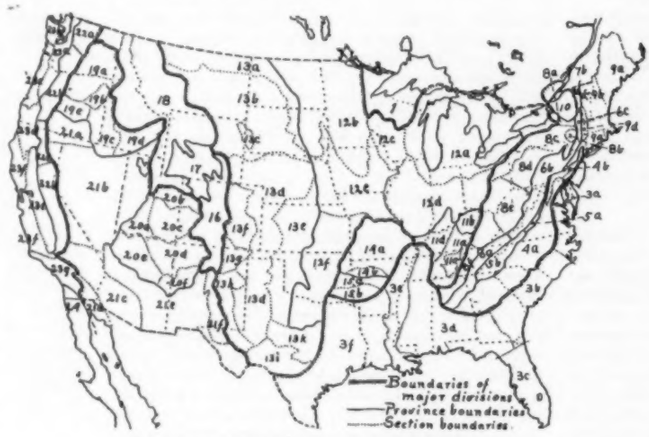
The basis of division shown on this map, here reproduced, is physiographic or, as might be said in Europe, *morphologic*. The divisions are based on land forms, not on climate or vegetation. If subdivision were carried far enough on the same principle each unit of the lowest order would comprise but one physiographic type. In most cases this has not been done. Even the units of the lowest order generally embrace several types closely associated in their development.

The genetic classification of land forms is now generally familiar to geographers, even to those who do not use it. In this system physiographic forms are classified according to their histories. Forms which result from similar histories are characterised by certain similar features, and differences in history result in corresponding differences of form. Generally the distinctive features which are important in a genetic classification are also obvious to the casual observer, but this is not universal. Thus a maturely dissected plateau may grade without a break from rugged mountains on the one hand to mildly rolling farm lands on the other. So also, forms which are not classified together may be superficially similar; for example, a young coastal plain and a peneplain. Hence this map, which distinguishes physiographic types based on a genetic classification, does not in all cases make the distinctions which are most obvious to the casual observer. On the whole, however, this discrepancy is not great. A very large proportion of all the boundaries shown on this map are familiar features. To have based the divisions purely on superficial features in proportion to their magnitude, would not have resulted in the making of units suited to scientific treatment.

Important physiographic differences between adjacent areas are, in a large proportion of cases, due to differences in the nature or structure of the underlying rocks. Where this is the case the two areas are distinguished on the geologic as well as the physiographic map. Distinctions based on geologic age also correspond to physiographic distinctions where the forms are so recent as to be in their first erosion cycle, as is generally the case with sheets of glacial drift. When these facts are remembered, it is not surprising that a large proportion of the boundary lines shown on the accompanying map are also geologic lines.

The segments here presented are of three orders, called respectively major divisions, provinces and sections. The basis of distinction among coördiate units is very much the same in all the orders. On the whole it may be said that contrasts in structure are stronger and more general between adjacent major divisions than between adjacent divisions of lower orders. Naturally also, the degree of topographic homogeneity is greatest in the units of the lowest order, but the reasons for calling one area a major division and another a province or a section are not clearly defined.

The degree of homogeneity in the several divisions of the same order is not in all cases the same. Homogeneity is strong in the Dissected Till Plains (12-c) which are practically everywhere submaturely dissected plains of moderate relief; also in the Snake River Plain (19–d) which is everywhere a young lava plain. On the other hand, the East Gulf Coastal Plain (3–d) is a heterogeneous area, for it grades from a young marine plain with undeveloped drainage near the coast to a maturely dissected, belted coastal plain farther inland; this case illustrates the inclusion of several types in one section by graduation, where no good dividing line is known, and where practical convenience requires that the types be considered in their mutual relations. Again, the Nevada Basin (21–b) comprises isolated mountain ranges (probably dissected block mountains) separated by aggraded desert plains; here is an intimate intermixture of several types which are, however, so related genetically that both are accounted for by the same history. Indeed, while this work has no direct reference to teaching, there is something almost final about the requirement that a province or section should be a suitable unit for scientific treatment. This is quite as necessary from the standpoint of government surveys as from that of the university.



Physiographic subdivisions of the United States

The committee distinctly disclaims finality for this work. With further investigation and more exact mapping some of the boundary lines here given may be shifted. The lines on this map were located by aid of the largest scale topographic and geologic maps available. Parts of the country are, however, imperfectly mapped, hence, with respect to exact plotting, the values of the several lines are unequal. All are necessarily generalized. As the result of future studies it may well be that the rank assigned to some of the units will be changed. Units of still lower orders will of course be made. Above all, the presentation of this map is not intended to preclude the use of other kinds of physical divisions like those of Supan, De Martonne, Herbertson, or Dryer. It is believed, however, that for a map of physiographic divisions, the main features of the one here presented will not be greatly changed.

The uses of such a map are of two general classes, (1) for scientific (explanatory) description, and (2) for comparative studies with other geographic elements. In the former, physical features are looked upon as the product of geologic processes, in the latter they constitute factors or conditions of life and human activity. In the former aspect they are an effect; in the latter a cause. The potency of such causes can only be known when statistics of population, agriculture and industry and even politics are graphically shown with due respect to natural units. It is plain that if matters statistical are to be represented on a map of natural divisions, and things human are to be discussed in terms of their physical setting, the value of the relations discovered will depend largely on the character of the natural divisions and their proper delimitation.

In the following table the names of major divisions are printed in italics; the provinces are numbered, and the sections lettered. The province number and the section letter correspond to those on the map.

Table of physiographic divisions of the United States

Laurentian Upland.—

1. Superior Upland

Atlantic Plain.—

2. Continental Shelf
3. Coastal Plain: (a) Embayed section; (b) Sea Island section; (c) Floridian section; (d) East Gulf Coastal Plain; (e) Mississippi Alluvial Plain; (f) West Gulf Coastal Plain.

Appalachian Highlands.—

4. Piedmont province; (a) Piedmont Upland; (b) Triassic Lowland.
5. Blue Ridge province: (a) Northern section; (b) Southern section.
6. Appalachian Valley province: (a) Tennessee section; (b) Middle section; (c) Hudson section.
7. St. Lawrence Valley: (a) Champlain Valley; (b) Northern section.
8. Appalachian Plateaus: (a) Mohawk section; (b) Catskill section; (c) Allegheny Plateau (glaciated); (d) Allegeny Plateau (Conenaugh section); (e) Kanawha section,[3] (f) Cumberland section.
9. New England province: (a) New England Upland; (b) White Mountain section; (c) Green Mountain section; (d) Taconic section.
10. Adirondack province.

Interior Plains.—

11. Interior Low Plateau[4]: (a) Highland Rim Plateau; (b) Lexington Plain; (c) Nashville Basin, (d) not named.
12. Central Lowland: (a) Eastern Lake section; (b) Western Lake section; (c) Wisconsin Driftless section; (d) Till Plains; (e) Dissected Till Plains; (f) Osage Plains.
13. Great Plains: (a) Missouri Plateau (glaciated); (b) Missouri Plateau (unglaciated); (c) Black Hills; (d) High Plains; (e) Plains Border; (f) Colorado Piedmont; (g) Raton section; (h) Pecos Valley; (i) Edwards Plateau; (k) Texas Hill section.

Interior Highlands.—[5]

14. Ozark province: (a) Salem-Springfield plateaus; (b) Boston Mountains (plateau);
15. Ouachita province; (a) Arkansas Valley section; (b) Ouchita Mountains.

Rocky Mountain System.—

16. Southern Rocky Mountains (to be divided into sections).
17. Wyoming Basin.
18. Northern Rocky Mountains (to be divided into sections).

Intermontane Plateaus.—

19. Columbia Plateau: (a) Walla Walla Plateau; (b) Blue Mountain; (c) Payette section; (d) Snake River Plain; (e) Harney section.
20. Colorado Plateaus: (a) High Plateaus of Utah; (b) Uinta Basin; (c) Canyon Lands; (d) Navajo section; (e) Grand Canyon section; (f) Datil section.
21. Basin-and-range province: (a) Oregon lake section; (b) Nevada Basin; (c) Sonoran Desert; (d) Salton Trough; (e) Mexican Highland; (f) Sacramento section.

Pacific Mountain System.—

2. Sierra-Cascade Mountains: (a) Northern Cascade Mountains; (b) Middle Cascade Mountains; (c) Southern Cascade Mountains; (d) Sierra Nevada.
23. Pacific Border province: (a) Puget Trough; (b) Olympic Mountains; (c) Oregon Coast Range; (d) Klamath Mountains; (e) California Trough; (f) California Coast Ranges; (g) Los Angeles Ranges.
24. Lower California province.

*From the Proceedings of the National Academy of Sciences. Communicated by W. M. Davis.

[1] An excellent account of these attempts has been given by Joerg, W. L. G., *Assoc. Amer. Geogr., Annals*, 4, 1914 (55–84), 22 maps.

[2] Fenneman, N. M., *Assoc. Amer. Geog., Annals*, 4, 1914 (84–134), 3 maps.

[3] Likewise part of the Allegheny Plateau.

[4] In the report of the committee this is called the Highland Rim Province. Messrs. Campbell and Matthes do not concur in the excision of this entire province from the Appalachian Highlands. They would divide it, and assign the eastern half to the Appalachians.

[5] In the report of the committee this is called the Ozarkian Highlands.

Removal of Carbon Bisulphide from Coal Gas

GAS coke, lignite coke, coke from sewage sludge, and kieselguhr were found to be incapable of absorbing carbon bisulphide from coal gas, and only a very small quantity was absorbed when the gas was passed over powdered retort carbon, boghead cannel, or hard pitch, or sawdust. By means of wood charcoal in small pieces both carbon bisulphide and hydrogen sulfide were removed to a considerable extent. For example, using 1 kilo. of charcoal per 5–6 cb. m. of gas, the total sulfur content of the gas was reduced from 73 to 20 grms. per 100 cb. m. The charcoal after use could be revivified by heating at 105° C. for two days.—*Note from Jour. Soc. Chem. Ind. on an article in Chem. Zeit. by H. Wanner.*

Recent Developments of Molecular Physics*

WHEN Lord Kelvin delivered his last Royal Institution discourse on April 27, 1900, his subject was: "Nineteenth Century Clouds on the Dynamical Theory of Heat and Light." Two new theories have since been advanced to disperse these clouds, revolutionary conceptions, not generally accepted, but commanding the serious attention of all mathematical physicists. To review these "Recent Developments in Molecular Physics" was the object of the discourse recently given at the Royal Institution, by Professor J. H. Jeans, F.R.S., of Cambridge, for some years at Princeton University, New Jersey, now residing in London.

The first cloud with which Professor Jeans dealt concerned the theory and aberration of light. The astronomer knew, he said, that he did not really point his telescope at the star he was observing; the man in the street was not holding his umbrella in the direction of the falling raindrops. The astronomer could watch the relative change in the position of a star in the course of years. In making his estimates he had to compound the velocity of light with the velocity of the earth. The earth might be supposed to sweep through the aether—there was another possible assumption—overtaking the ripples or light waves coming on and leaving them behind afterwards. It was somewhat the case of the man in the boat; going up-stream he overtook more ripples than going down-stream, and counting the ripples either way, he might arrive at an estimate of the rate of the river current. Similarly, it ought apparently to be possible to estimate the aether drift past the earth from the difference in the velocity of light measured first in the direction of the motion of the earth and then in the opposite direction. All the various experiments made had, however, failed to disclose the existence of any such difference; thus it was concluded, either that the aether had no velocity relative to the earth, or that we were unable to detect it. The theory proposed to disperse this cloud overshadowing the dynamical theory of light, viz., the theory of relativity, started from the supposition that science had no means of deciding whether the aether was at rest or in motion, since we can only measure relative motion. That theory, therefore, took away the function of the aether, and it did not matter whether the aether existed or not—a point to which the lecturer reverted. It might be objected, Professor Jeans continued, that the removal of that cloud only brought up others. But the relativity theory actually cleared up other difficulties. Einstein had recently extended the relativity theory to comprise the laws of gravitation, which stood apart from all other laws, as unexplained, and it resulted that the law of inverse squares was only a first approximation. Professor Jeans alluded only to one exemplification in this connection, the case of the planetary orbits. The planets did not merely move in ellipses, but those ellipses seemed to rotate in the direction of the movement. Einstein had calculated that, in the case of Mercury, the nodes should move by 42.9 seconds of arc per century; the observed, but hitherto unexplained, value of astronomers was 43 seconds.[1]

The second cloud, Professor Jeans proceeded, concerned the dynamical theory of heat and the law of the equipartition of energy. This latter law (due to Maxwell, developed by Boltzmann and Rayleigh) was true in many cases, but had been found not to agree with all the observations. To understand the meaning of equipartition, one should think of bodies or systems capable of moving in different ways. If a point or atom could merely move to and fro, it was said to have one degree of freedom; if it could move in three directions it had three degrees of freedom of translational motion. There might further be vibrational and rotational movements, each with several degrees of freedom. The equipartition law now stated that after the disturbance had passed away and equilibrium conditions had been established, each degree of freedom, no matter of what type or family, would be found to have received from the disturbance the same amount of energy. That was true for gases like the air, in this sense, that the molecules of oxygen, nitrogen, helium, etc., had the same average energy. The most beautiful exemplification of the law was given by Perrin with regard to the Brownian movements of small particles of various sizes, suspended in liquids. The law also held for solids; for the atomic heat of all solids was approximately the same, subject to the reservation presently to be mentioned. But though applicable to molecules and atoms, the law did no longer hold for electrons, which had not all the same average energy.

Further to explain the discrepancy, Professor Jeans made use of other illustrations. When an organ pipe was blown it gave a fundamental note, and if blown harder, also the octave and other upper harmonics, representable by a series of tuning forks decreasing in size but increasing in frequency of vibration. When the wave-length was as small as the molecular distances the movements became a pure heat vibration, and the heat energy of a gas could be resolved into the energy of regular sound waves. That applied also to a solid, a string or wire vibrating as a whole or with one and more nodes in waves of decreasing length and increasing frequency.

In accordance with these arguments the atomic heat of all solids seemed to be the same and independent of temperature. That, however, recent low-temperature researches proved, was true only for ordinary temperatures about the neighborhood of 300 deg. C. absolute. As the temperature sank below 200 deg. absolute the atomic heat decreased, more or less rapidly at first, and finally very slowly again as absolute zero was approached. The atomic heat curves for silver, copper and aluminium were exhibited. Thus the energy partition was not really the same, and the question was: What kind of partition of energy will yield the observed curves? By adjusting the scales of these curves P. Debye (Leyden) had succeeded in making all the atomic heats of these three (and also of other) bodies fall on one and the same curve; this theoretical curve was deduced from Planck's formula for the radiation of a black body. In this curve of Debye the specific heat of any (monatomic) solid appeared as function of T/θ, where T was the absolute temperature and θ a temperature characteristic of the element; θ could be calculated from the elastic constants, and T/θ was equal $1/x$, where $x = h\nu/RT$, h being Planck's universal constant, ν the frequency and R the gas constant. The total heat energy of a solid could be estimated from a knowledge of the specific heats at all temperatures, and this energy was exactly accounted for by assigning an average value of $R\,T.\,x/(e^x-1)$ to each elastic vibration of a solid.

The explanation of Debye's curve and the general graph $(x/(e^x-1)$ was that the equipartition was not a universal law and not of absolute uniformity, the reason being that the equipartition theory was based upon the classical (Newtonian) laws of motion, which did not hold for low temperatures and high frequencies. The x became small for small ν and for high temperatures T; at low T, x was very large, and the amount of energy per degree of freedom became very small, as the study of the black-body radiation had shown. As stated, Debye went back to Planck, whose method of deduction had been criticized. But one could work back from the experimental curves, and Poincaré had shown that the experimental results inevitably led back to the $R\,T.\,x/(e^x-1)$, and this in turn inevitably led back to the quantum theory. The lecturer himself had in vain attempted to explain the radiation laws on the basis of classical mechanics. Yet there was something very striking in the suggestion—which was the essence of Planck's quantum theory—that there could be no gradual continuous increase in energy, but only an increase in jumps, in whole multiples of $h\nu$. That suggestion was not so strange as it appeared, however. One had only to consider that under sufficient magnification no solid appeared to be continuous.

But the nature of the jump was more open to doubt, and the first question was, of course: Is there any direct evidence of energy in definite quanta or bundles? There was. When a polished metal was illuminated, nothing happened unless the frequency of that radiation exceeded a certain minimum value. Then only did electrons come off, the atoms of the metal being broken up. One might imagine that any light of lower frequency, if only of sufficient intensity or of sufficient duration, would have the same effect; but that was not so; quality, not quantity, was required. The atomic structure might be likened to that of a net of fine mesh; millions of small fishes could pass through without causing a disturbance, but one big fish would tear the net. When an atom was broken up, a certain amount of energy was spent in the ionization, which sent off an electron at a certain measurable speed; the two amounts together made up one quantum of energy.

Thus the cloud that had prevented an explanation of photo-electric and other effects on the old views had been removed. But there were the optical effects, interference, diffraction, etc., which seemed to demand a continuous front of light waves, quite incompatible with energy radiation in separate bundles. It was, in fact, very difficult to reconcile the quantum hypothesis with the observed optical phenomena, for which the undulatory theory accounted. Before the quantum hypothesis was developed J. J. Thomson had himself suggested that the energy of light was not uniformly spread over the whole wave front, but concentrated at particular points.[2] The simplest way of attempting a reconciliation was perhaps to abandon the aether altogether, and to rely on the theory of relativity. But that explanation, again, did not go the whole way. On the other hand, the spectra of the elements and the arrangements of the lines in series (observed in the various regions of the spectrum) had been successfully approached on the quantum theory. This study had been taken up by N. Bohr in his "Attempts to Develop the Constitution of Atoms and Molecules on the Basis of Planck's Hypothesis for Explaining Black-Body Radiation." Bohr's atom consisted of a positively charged nucleus surrounded by a ring (in the case if hydrogen) or rings of electrons rotating about the nucleus in orbits of variable diameters. If some cause—for instance, the electric discharge in a vacuum tube—had removed the electron from the nucleus to a great distance, the atom would be re-formed by the binding of the electron, and the total amount of energy radiated out by the passing of the system from one of the stationary states to another corresponded to multiples of $h\nu$, while the angular momentum of each electron in the permanent state was $h/2\pi$. The whole conception of this type of atomic structure, which general considerations had suggested, was only intelligible if the energy radiation was discontinuous—not continuous—and from such considerations Bohr had correctly calculated for many spectra the Rydberg constant (which was the same for all the different spectrum series), the ratio e/m (charge to mass of the electron). That conception of the atom had been carried further by J. W. Nicholson, who had indeed predicted the structure and spectra of unknown elements, the hypothetical coronium (a line in the sun's corona) and nebulium (in nebulae). The Planck hypothesis thus had a large amount of experimental evidence to support it, and Professor Jeans certainly thought that there was something in it. Yet science was in a quandary. But very considerable progress had been made, and the new view of discontinuous motion should be followed up.

*From *Engineering*.

[1] We should refer our readers as to this feature and the relativity theory to *Engineering* of August 11, 1916, page 132, and September 29, page 297.

[2] These views and the difficulties of the quantum theory were discussed by Sir J. J. Thomson last year; see *Engineering*, vol. ci, page 380, April 21, 1916.

The Religious Ideas of the Northwest Coast Indians

Dr. Frachtenberg stated that four important features of the religious ideas noted among the Indians of this region are: (1) An intensive animism; (2) a belief in the powers of supernatural beings as dwarfs and giants; (3) a belief in the existence of guardian spirits; (4) a complete absence of the social phase of religion.

According to Dr. Frachtenberg many religious ideas are common to all the tribes of the Northwest Coast, yet the northern and southern portions of this area are found to differ in cosmogont. The tribes in the extreme southern portion believe that the world was created out of a watery mist, the Transformer enlarging a small piece of land until it became large enough for habitation. The tribes of the northern portion are satisfied with a world whose origin is not explained. They hold, however, that the Transformer (Creator) first made man and the members of faunal and floral kingdoms, and at a later time returned and improved this creation. Two visits of the Transformer are typical of this region. In the south the Transformer (Creator) and the Trickster are separate individuals, while in the north they are unified. In the south the Transformer makes all that is good, and the Trickster is held responsible for all the bad elements; while in the north there is no such dissociation. Good and evil things were alike created by the Transformer.

The Northwest Coast Indians believe that an individual comprises a body inhabited by two "souls" and a "ghost." In a slight illness the "outer soul" becomes spearated from the body, in a serious illness the "inner soul" wanders to the "country of souls" but may be recalled by the shaman. When death occurs the "ghost" also departs and the shaman has no further power. The "country of souls" contains two divisions, one inhabited by recently arrived souls, the other by souls which have been there for a longer time. On their journey to this land the souls pass a "rest-house," then a "lake" and a "berryground." Their way is barred by a "spring-pole," and they must cross a "rotten log," the final barrier being a wide river.

No ritual or systematic form of supplication is found among these Indians; indeed it may be said that guardian spirits take the place of deities. Every man and woman possesses one or more guardian spirits each of which has its special sphere of influence. Thus there are guardian spirits for securing good weather and plenty of seal or whale, guardian spirits for success in hunting, and for help in making baskets and canoes. No offerings accompany a request to a guardian spirit. Crude representations of these spirits are seen on the implements and on the house-posts of their owners.

The shamans receive their power from a multitude of spirits. Certain shamans are considered to have power to cure sickness, while it is believed that others can "steal a man's soul," causing either serious illness or death.

Graphic Charts That Mislead

Faulty Constructions that Obscure Facts

By John Wenzel, Graphical Statistician

DURING the last few years the use of graphic charts has been constantly increasing. While there has been a marked tendency towards greater correctness in the form and style of diagrams which have appeared in books, magazines and papers, many of the published charts have not conformed to the accepted standards for the making of graphic charts, and are positively misleading.

That the zero line should be shown whenever possible, is a fundamental rule that has been laid down for the making of graphic charts, and yet this rule is frequently broken by those who are engaged in the graphic presentation of facts. Cutting off all the horizontal rulings below the curve line, as is often done, may save space, and the cost of the plate may be reduced if the chart is intended for publication, but by taking away the zero line the correct interpretation of the chart is hindered, and impressions may be created which are misleading. Failure to understand the true significance of the curve line is undoubtedly responsible for the frequent omission of the zero line. If it is desired to show graphically the population of the United States by decades during the last 50 years,

YEAR	POPULATION
1860	31,443,321
1870	38,558,371
1880	50,155,783
1890	62,947,714
1900	75,994,575
1910	91,972,266

one of the simplest methods is to use vertical bars as in Fig. 1, the heights of the respective columns representing the population at different periods according to the scale

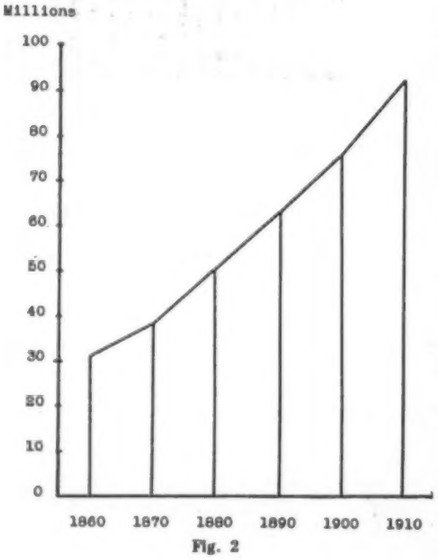


Fig. 2

at the left. As the eye travels along the tops of these columns, it unconsciously judges their relative heights and constructs an imaginary curve which coincides with the tops of the columns.

Instead of a heavy black column representing the population of each decade, a line could be used, the tops of these lines being joined with a curve as in Fig. 2. The line joining the tops of the 1870 and 1880 columns sloped upward at a greater angle than the line joining the tops of the 1860 and 1870 columns because the increase from 1870 to 1880 was four and a half millions more than was the increase from 1860 to 1870. From 1880 to 1890 the increase was only about a million more than it was from 1870 to 1880, hence the variation in the slope of the curve between these two decades was slighter. From 1890 to 1900 the increase was about three million more than during the previous decade. The slope of the line joining the tops of the 1890 and 1900 columns therefore was a trifle more steep. The increase from 1900 to 1910 was also about three millions more than the increase from 1890 to 1900 so that the upward slope of the curve was about the same as it was during the previous decade. This curve by visualizing the population for each respective decade helps the mind to grasp at a single glance the comparative increase of population.

The next step in the development of the curve line is shown in Fig. 3. Here upon a field with both vertical and horizontal rulings, points marked with an X, representing the population of each successive ten-year period,

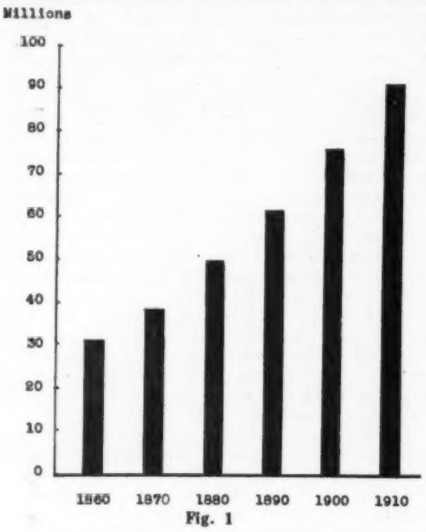


Fig. 1

are located on the vertical rulings according to the scale at the left and correspond with the heights of the respective columns and lines in Figs. 1 and 2. By joining these points the curve line becomes the most prominent feature of the diagram.

As the population in 1860 was 31,000,000 and in 1910, 92,000,000 the former was approximately one-third of the latter. Measurement shows the distance from X to the zero line in 1860 to be very nearly one-third of the distance from X to the zero line on the 1910 ruling. The ratio expressed mathematically is approximately 31:92::1:3. The length of the bars in Fig. 1 and the vertical lines in Fig. 2 are relatively correct and convey to the reader a truthful impression of the number of persons who were living in the United States at the times when the census enumerations were made. Likewise the distance from the zero line to the respective X's gives a correct presentation of the actual population, and thus shows the relative growth from decade to decade.

If in Fig. 3 the three lowest horizontal rulings were omitted and the thirty million line were used as the base line as is shown in Fig. 4, the curve would be identical with that in Fig. 3, but there would be no accurate means of comparing quickly the 1860 population unless an imaginary zero line were constructed as far below the thirty line as the thirty line is below the sixty line, as in Fig. 5. Thirty millions have been cut off of each column thereby practically destroying the measure for comparing accurately the population of one decade with

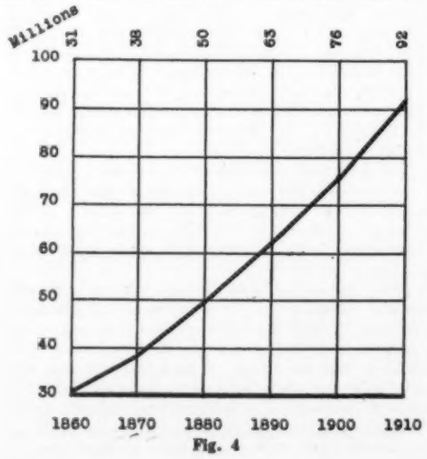


Fig. 4

that of another, for few people will take the trouble to construct an imaginary zero line as in Fig. 5.

Fig. 4 has been mutilated and distorted as much as Fig. 1 would be, were 30,000,000 cut from the bottom of each column, making Fig. 1 look line Fig. 6. Returning to the proportion 31:92::1:3; if thirty is subtracted from the first two numbers, the result obtained 1:62::1:3, is incorrect, the last figure being almost twenty times too small. These figures emphasize the importance of the zero line and give conclusive proof of its necessity for the correct interpretation of a graphic chart.

It seems hardly credible that a financial house which sells industrial securities should wilfully misrepresent the sales of one of the companies whose stock it was trying to sell. Nevertheless, if anyone saw the diagram shown in Fig. 7, he would be given an entirely erroneous impression of the difference between sales for January, 1913, as shown in the left-hand column and the sales of October, 1916, as represented by the column on the extreme right.

Only after referring to the scale at the left will it be seen that instead of starting at zero, the scale begins at 130 millions and ends with 240 millions so that the short column at the left which is about one one-hundredth as long as the column at the extreme right, really stands for 131 millions or more than half as much as the column at the right which stands for 238 millions.

Although the volume of sales for each quarter appears in figures at the top of each column the reader may get the real facts only by a most careful examination of the scale and the figures for each quarter. The real function of the graphic chart is to present facts so that the whole story may be told at a glance, and a hasty

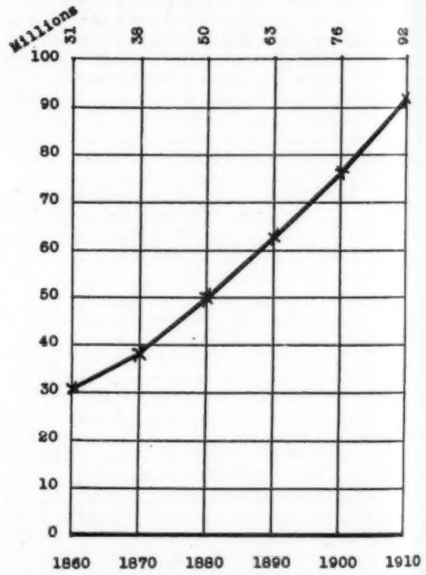


Fig. 3

glance at this diagram would hardly fail to create a wrong impression.

In Fig. 8 the diagram has been redrawn so as to show the correct method of presenting the facts. In this diagram the proper relationship between the sales of January, 1913, and October, 1916, is shown and the impression conveyed is very different from what Fig. 7 would have you believe. In Fig. 8 the scale has been made about half as large so that the two diagrams might be approximately the same size. In this the base line begins at zero instead of at 130 millions. The bar representing the January, 1913, sales is more than half as long as the bar representing the October, 1916, sales which conforms with the actual facts. At first glance the growth as shown in Fig. 8 does not appear to be so great as it appeared to be in Fig. 7, and yet both diagrams present graphically exactly the same data. Fig. 7 shows it in a way which, if not intended to mislead, is likely to do so unless examined with great care. Instead of presenting the real facts so they can be easily and instantly understood, this diagra.n shows them in a

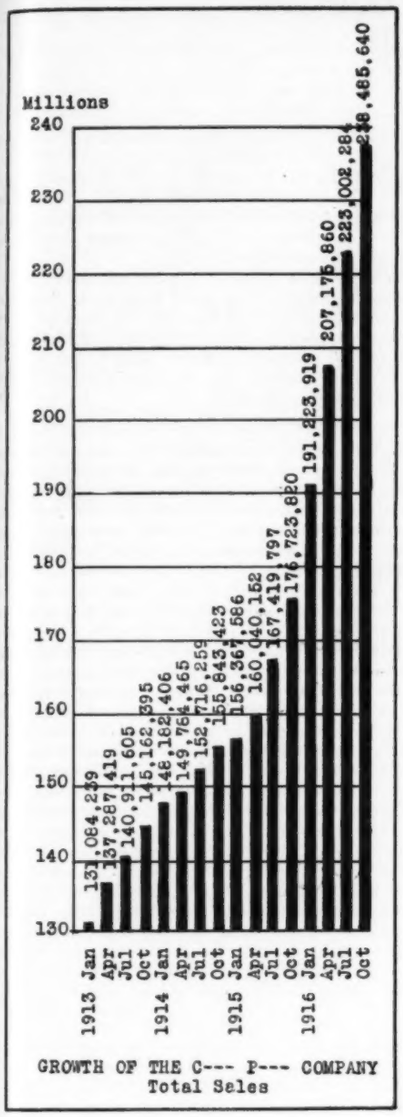


Fig. 7

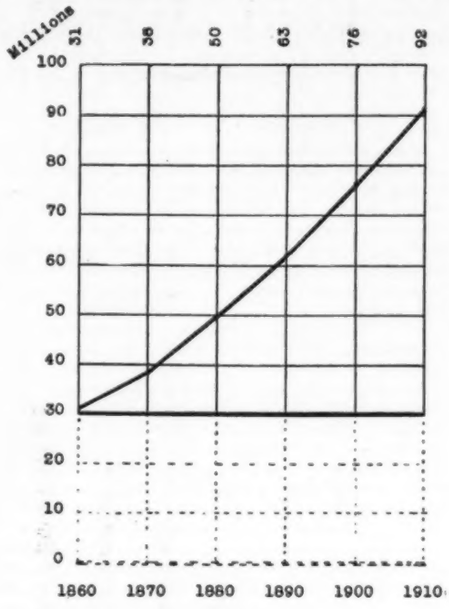


Fig. 5

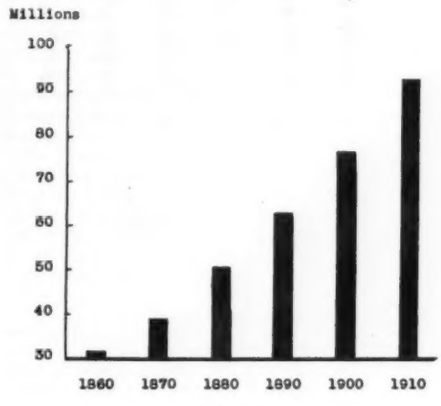


Fig. 6

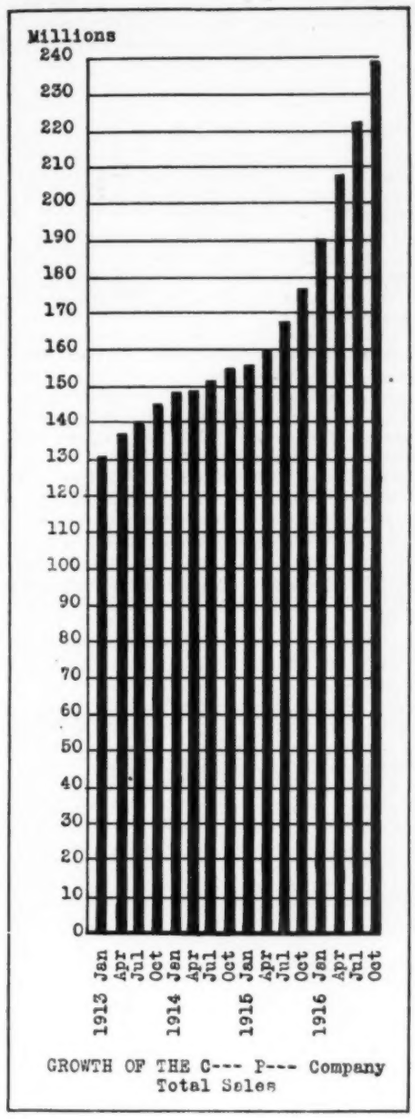


Fig. 8

way very likely to be misinterpreted and this is far from the proper function of the graphic chart.

If it will be borne in mind that it is the distance from the top of the bar or from the curve to the zero line that is really significant, then the zero line will be considered as important as the length of the bar or the curve itself, and in drawing a diagram a scale will be chosen that will permit the whole field to appear within the space allowed for the diagram.

A Motor Ambulance Driver's Notes from the Front*

By R. Cartwright[1]

One sees strange sights in France nowadays. When driving over a level-crossing the other day, not a hundred miles from Paris I saw, wonder of wonders, a dingy looking S. E. and C. R. engine puffing away with a decrepit coach attached. They are all here, old Metropolitan engines now enjoying a real fresh air cure after their murky courses on the old Underground; Welshmen from the little Cambrian lines; L. and S. W. R. express engines; in fact, all the good old puffing locos. whose arrival at the scrap heap has been postponed till they have seen the last of the Boche in France.

Motors must far surpass rabbits in fecundity. Roads bristle now with all manner of types; rivers and canals get their share of motor boats, while even the rails sometimes gives passage to a petrol motor-driven train. These "promoted" road plants do not aspire to be heavy tractors, but they can cover the ground and are always ready for any special use. Farther up the railway line the rail gages become narrower, but that does not worry these adaptable petrol outfits. A bolt is drawn from each axle and the chassis is run along a diminishing line until another bolt hole is reached, when each wheel is again locked to the axles, and away they go right up to the Front without any need to displace the comparatively light loads carried on the trains. This is a French idea, and is in use on the Verdun Front, where, although home newspapers do not take the trouble to give more than a passing reference to it, much is happening daily.

One meets or hears of most of the French motor car agents who used to come along to Olympia. They are naturally either in transport or air service, but it is good to shut one's eyes and fancy again that cars of all sizes are around or that the roar of guns is only the unsilenced exhaust of a demonstrating car. It is possible to do this when sitting drowsily beside a charcoal bucket and with a motor car merchant in most beautifully cut blue talking big business about Panhards; that is to say when his companion, a Berliet specialist, will allow him breathing space. One of these days I expect an automobile expert will officially review the automobile section of the war transport; his opinions will certainly form interesting reading.

Among the prisoners brought back by the French the other day was a man who had driven tourists over every inch of the North of France. He knew the topography of the country perfectly, especially the location of buildings suitable for military needs, and actually had on him a road map marked for staff purposes. It was a strange world we innocents lived in before the great war; spies everywhere to watch and tabulate everything for our undoing. Nothing escaped the notice of the Hun, whose dossiers were complete almost to the last spanner contained in the manufacturing districts. This particular man had marked in red all the places where good machine tools were available; he had even made special mention on the reverse of the map of those places where repair posts could be organized, and, generally speaking, seemed to have at his finger ends details of all the available space and accommodation hereabouts for motor transport.

A typical Hunnish spy he was, too—a good linguist, a sound tradesman, of urbane demeanor and smart appearance. He was under some constraint, when first captured, as to what his fate might be. He was unfortunate enough to be recognized by the Parisian doctor who attended his wound, but he got through on the ground that his spying was all done before the war; after all, he is only one of thousands similarly situated.

Scattered about the ground over which we run our ambulances we see the riven chassis of poor old broken cars. The skeletons are now carefully collected, and out of the good parts of these weatherbeaten wrecks the modern miracle-working French mechanic manages to evolve many a presentable machine. The absolute scrap goes back for melting down, for nothing is wasted. On passing one of these dumps I feel a little dismal, as I cannot help thinking that the war-broken cars there represented might possibly—had things happened otherwise—have included the remnants of my own car, for one comes to love one's car when one has practically lived in it and found it never failing.

*From *The Autocar*.

[1]Mr. Cartwright is a private motorist who has been driving a motor ambulance in the service of the French Red Cross since the autumn of 1914.

Cotton and Wool Finishing Processes*

Problems of Chemistry, Physics and Mechanics

By W. Harrison

VARIOUS chemical substances are used in finishing textiles, but the processes are mainly physical. It must be admitted that few attempts of any kind have been made to place finishing on a scientific basis, and like photography, the art is in advance of the science. With the majority of fabrics two things have to be considered: first, the appearance, and second, the feel. Strictly speaking, there is a third consideration, the warmth or heat-retaining power. Other properties are, of course, required in the case of waterproof and fireproof fabrics.

Apart from the color, fabrics possess an appearance which is of great importance, but until some standard method of describing finishes is devised, it is impossible to describe the numerous types of appearance possessed by fabrics, except by referring to the fabrics themselves. The appearance of a plain calendered or pressed material is familiar, and is easily understood, since it is similar to that of a flat surface of metal. It is not so easy to understand the appearance of lustrous materials.

CAUSE OF LUSTER

Luster is due to reflection of light more in some directions than in others from a large number of small surfaces, as distinguished from metallic reflection, which is all from one surface. In schreinering as the fineness of the line increases, say from 20 to the inch, it becomes more and more difficult to see individual lines. As soon as the limit is passed where separate lines are not detectable by the naked eye, the result is luster. The usual number of lines is about 250 to the inch. If the lines were 5,000 to the inch, the result would still be luster. A non-lustrous fabric is not one which does not reflect light at all, but one which reflects the light in all directions and diffuses it.

The luster of silk is largely due to internal reflection, as one can see by turning a fiber while held under a light. The same applies to artificial silk and certain kinds of mohair. There is reflection from the external surface of these fibers as well as internal reflection. By means of polarized light, it is quite easy to differentiate between surface reflection and internal reflection, the former being almost completely polarized, the latter only to a slight extent.

In mohair lusters and in mercerized cotton both internal and surface reflection are produced, but the internal reflection is not so marked as with silk. The magnitude of the internal reflection is dependent on the shape of the section of the fibers. A perfectly round section gives very little reflection from the internal surface; a slightly oval section gives a large amount; while a flattened section gives only a little.

CHANGE IN FIBERS

Before attempting to explain the effect of the various finishing operations on the feel of fabrics, a few facts on colloid chemistry will be given. The class of colloids to which textile fibers belong have many important properties in common. Gelatine, when placed in water, swells, but the swelling does not proceed indefinitely until the water is heated. Starch acts somewhat like gelatine, but the swelling is very small although noticeable in cold water. Cellulose also swells in cold water, but to an extremely small extent, and in fact is only small in hot water. The changes produced in hot water are, however, recognizable as due to swelling. In caustic soda, the swelling is much greater and in cellulose solvents, indefinite. Wool and silk behave in a similar manner to cotton, but the swelling is assisted by acids. In boiling water, wool swells more than cotton, but even then it is only small. The reverse change, that is, from the swelled condition to a more solid condition, is termed "consolidation," for convenience.

All these colloids have a similar property, in that, after they have been swollen with water, subsequent consolidation does not always put them into the same condition as before swelling. The nature of the change depends on the manner in which consolidation has been produced, the important factors in this respect being the temperature, pressure, degree of moistness and time of treatment.

Practically all processes of finishing are essentially methods of changing the colloidal state of fibers, and consist in, first, swelling them either by wetting in cold or hot water, damping, or steaming with wet steam; and second, in consolidating them by treatment under various conditions of temperature, pressure and time. In all cases the final result is influenced by the method of spinning the yarn and by the nature of the weave.

*The Textile World Journal.

COMBINATIONS IN FINISHING COTTONS

In hot calendering and schreinering cotton, the goods are swelled by damping and consolidated by treatment at high temperature and high pressure for a short time. In hot pressing, the cotton is treated at lower pressure and lower temperature for a much longer time. In beetling, the swelled cotton is treated at low temperature and high pressure for a long time. In a patented process, pressed cotton is exposed to a high temperature for a long time.

It is thus seen that all possible combinations capable of producing any considerable effect are used in the finishing of cotton. In mathematical language, one would say that the magnitude of the effect depends on the product of pressure, temperature and time. There is a limit to the temperature, above which the fibers may be tendered. The limit to the pressure is usually beyond that which can be applied. The limit to the time is controlled not always by the fabric, but by the question of cost.

The result produced by the treatment is different in each case, but, as will be explained later, it is a difficult matter to describe the differences; all one could do would be to produce patterns, and say that the differences in the feel are in accordance with what would be expected from colloid chemical knowledge. The more a material has been consolidated, the harder the feel. It has been proved that mercerization changed the colloidal state of cotton in a manner represented by the processes of swelling.

The materials used in finishing cotton are substances which undergo similar changes to the fiber, but to a greater or less extent. The change is greater for starch alone, but may be less when softeners are added. So-called permanent finishes on cotton are those in which the cotton has lost most of its property of swelling in hot water or in wet steam. This result is obtained by treatment at high temperature with or without the application of pressure, which produces changes entirely in accordance with what would be expected from the colloid-chemical properties of cotton. There is, of course, the process of waterproofing the cotton after finishing, so that the water cannot get to the fiber to cause swelling, but this is a special case.

FELTING AND SHRINKING WOOL

In wool finishing, the main processes are raising, felting, shrinking, pressing and setting. Under the term setting is included boiling, crabbing, blowing and steaming, followed by cooling. The process of raising is mainly mechanical, although physical properties of fibers are of importance. The process of felting is generally supposed to be due to interlocking of the serrations of the wool fibers, but the author does not agree with that theory. If the serrations can be made to fit so easily by working in a machine, there is no reason why they should not be separated just as easily unless some other factor comes into play. This other factor is the swelling of the wool, which causes the fibers to stick together when subjected to a sharp blow. This is another colloid-chemical problem.

Shrinking, although occurring during felting, is quite distinct from it, and measurements of the amount of shrinking do not give a true indication of felting properties. Shrinking is caused partly by the curling of the yarn and partly by the curling of the individual fibers when put in water. The latter effect is due to strains within the fiber, which are allowed to act when the fiber is softened. In setting, we have the corresponding physical process to the mercerization of cotton under tension; the strains are arranged parallel to the fibers, so that curling no longer occurs. The author has been able to prove that the strains are more regular after the fibers have been set. Shrinking of the fibers themselves is a slow process, but is not prevented by the process of setting.

In cold pressing of wool the goods are swelled by damping and consolidated by pressure acting for a long time. In hot pressing, the temperature is higher and the time less. In the rotary press, the temperature is still higher and the time still shorter. In the finishing of wool fabrics generally, more attention is given to high pressure than to high temperature, since wool is more readily damaged by the action of heat than cotton. As a matter of fact, high temperature is not so necessary since the colloidal state of wool is more readily changed than that of cotton. We may say that the product of temperature, pressure and time may be smaller with wool than with cotton. The final result will depend on the magnitude of each of these factors as well as on any previous treatment the wool may have received. Scouring, bleaching and dyeing all have an effect on colloidal state. The so-called permanent finishes on wool are obtained in a similar manner to those with cotton, usually by high temperature treatment producing more or less permanent consolidation.

The differences in raw wool are much greater than in cotton, for there may be many different qualities even on the same sheep. All these different qualities, in the author's opinion, represent different colloidal states, and for this reason the different qualities are likely to require different treatment. However, these are theories which remain to be proved.

TEMPERATURE AND PRESSURE MEASURED

In carrying out investigations on change of colloidal state, one determines the changes produced by definite temperatures, degrees of moistness and time of treatment. This is the scientific method of working in the laboratory, and it ought to be the method of working in the mills. As a matter of fact, in the mills few or no attempts are made to measure temperature, pressure and moistness. Time is taken into account, but not entirely on account of its importance to the result.

The practical man will say that he can get temperature and pressure quite near enough to produce a certain finish. In the case of experienced finishers, this is no doubt true, but there are many objections to the principle of guessing. In the first place, a little health trouble may completely spoil a finisher's judgment, with subsequent damage to the material. In the second place, no records can be kept of how a finish has been obtained, and this may in certain circumstances be a source of trouble. But in the third place, and this is considered by the author to be the most important, it is impossible to get the best advantage out of assistance from the scientifically trained men, since they have no means of applying their knowledge successfully to the process of finishing. In order that the scientifically trained man may help manufacturers and practical men, they must be provided with the necessary scientific tools to work with, and, in finishing, these are thermometers, pressure gages and other gages.

WATERPROOFING FABRICS

In the waterproofing of fabrics, the processes can be divided into two classes: one in which the fabric is coated with a material impervious to water, such as rubber, and the other in which the fabric is treated with a fatty material which repels the water. Only the fabrics treated by the first class of process are really waterproof. It is quite easy to make a fabric hold water in its folds, in fact, one can do the same with a fine sieve by greasing it, but it does not follow that it is really waterproof. For wearing purposes, a fabric is sufficiently waterproof if it will not allow water to pass through when in a heavy rainstorm. So far, no satisfactory standard test for waterproofed fabrics has been designed.

The fireproofing of fabrics must be considered as a special problem, and will not be dealt with here.

NEED STANDARD METHOD OF DESCRIPTION

There are many difficulties to be met in trying to put the processes of finishing on a scientific basis. It is, of course, essential that some means be devised for defining the nature of a finish, and that will require some standard tests. It may be possible to devise standard tests for the degree of waterproofing or the degree of fireproofing of a fabric and also for the heat-retaining power, and it may also be possible to define the appearance of a fabric in quantitative terms. But when one comes to consider the "feel," the difficulties begin to increase.

The sense of sight is fairly exact with all individuals and it is possible to describe what can be seen. The sense of touch, on the other hand, is usally inexact. An experienced finisher has a well-developed sense of touch, but the author has not yet met the man who can describe the "feel" of a fabric in such exact terms that another trained man could imitate it without feeling the fabric himself. There are some instances where the sense of touch is very exact. For instance, it is possible to detect the difference in thickness between two pieces of paper, one a one-thousandth and the other a two-thousandth of an inch thick. Without the aid of a microscope, it would be difficult to detect the difference with the sense of sight.

The sense of touch is inaccurate in determining the actual wetness of a piece, for it is a well-known fact that a cotton piece feels wetter than a wool piece when both

contain the same percentage of water. The reason for this is that the temperature affects the judgment of wetness.

The judgment of temperature by the sense of touch is also inexact, for a cotton piece feels colder than a wool piece when both are at the same temperature but below the temperature of the fingers, whereas the cotton piece feels warmer when both are at the same temperature above that of the fingers. The reason for this is that the heat conductivity has a great effect on the temperature as judged by the sense of touch, and what one really feels is the amount of heat which passes between the fabric and the fingers in a given time.

The important question is, how to describe the feel of a fabric in exact terms. The practical finisher uses such terms as stiffness, paperiness, thickness, fullness, softness, leatheriness, springiness, smoothness, wooliness, silkiness. All these terms are relative and, although known to the finisher, they cannot be used to convey an exact idea of the feel, even to other practical finishers. It is far easier to describe a color than a particular feel.

The problem of describing the feel of a fabric in exact terms is a very difficult one, perhaps the most difficult in the whole of the textile trade. However, there have been so many so-called impossible problems solved within the last twenty years, that it would be unwise to leave the problem unsolved for want of experimental research. Certainly, a large amount of work would be required, and most of it would probably be useless, but there is reason to suppose that the problem could eventually be solved, though not without the application of other knowledge than that relating to textiles.

Chemicals for Laboratory Use*

Their Purity, and the Investigations Preparatory to Their Production

By William Rintoul

THE purity and reliability of reagents and materials constitute one of the main factors essential to accuracy in chemical analysis and research. In the past Germany has enjoyed a monopoly of the supply of these bricks and mortar of our science, and that we have been content that this should be so, provides an example of the grounds on which posterity may justly base its inevitable reproach and censure on our national neglect of science and of industries intimately connected with scientific development. Practically the whole of the energies of the limited number of chemists qualified for this work were occupied in carrying on those industries which had already been established and no spare force was available for extension. In fact, the chemists in this country were and are in the position of an army without reserves.

To this cause is due the neglect in this country of such industries as the manufacture of pure chemicals for laboratory use. So long as our energies were fully occupied in keeping alive existing industries, and so long as the quality of our supplies of imported materials remained satisfactory, we had no choice but to accept the position which had arisen. Of late years, however, the quality of supplies from these sources has been degenerating and this degeneration has been coincident with a material growth in importance of the industry involved. For these reasons, and because of the imperative necessity that in future we must be independent of Germany as far as possible, it is clear that the time has now arrived when the British Empire must consider her position with regard to the supply of materials necessary for the proper control of so many of her industries.

With one or two exceptions, the raw materials employed in the preparation of laboratory reagents, that is to say, a wide range of chemical substances of a commercial or technical standard of purity, can readily be obtained within the Empire, and if the scientific knowledge and skill necessary for the elaboration of the methods and plant requisite for their purification can be made available, there is little doubt that this growing industry need not long remain in foreign hands, in so far as this country's requirements at least are concerned.

The difficulties to be overcome in establishing an industry such as this must not be minimized. In the case of an entirely new industry, one is often hampered by the lack of available information and by the fact that each step has to be fully investigated before general principles can be established. In the case under consideration, the difficulties are of an opposite nature. An enormous literature has already grown up dealing with the preparation and properties of chemical substances, and unfortunately much that has been written is either inaccurate or misleading. This is true to such an extent that in many cases the published statements are embarrassing rather than helpful. Especially in text-books is this the case, probably because of the extensive copying which has taken place, writers of such books being rather prone to accept as correct statements that have appeared in earlier works. Even when a statement which has acquired weight by reiteration alone is disproved, it is sometimes extremely difficult to persuade an author of a subsequent text-book to take the responsibility of giving the corrected version the weight of his approval.

An instance of the misleading statements which find their way into the literature is to be found in the method for the preparation of dimethylaminobenzaldehyde described in one of the most recent German text-books. Repeated experiments, in which the directions given were most carefully followed, failed to produce the result claimed by the author, and it was only after a considerable amount of research that a successful method for the preparation of this substance was evolved. Again, in the preparation of phthalic acid, if, as is laid down in the literature, the mixture of naphthalene, sulfuric acid, mercuric sulfate, and water is slowly heated, nearly fifty per cent of the naphthalene distils unchanged and a low yield results, whereas if the naphthalene and sulfuric acid are first of all heated together until sulfonation is complete and then the mercuric sulfate and water are added, a much more economical yield is obtained.

At the outset, therefore, when the reagents to be prepared have been selected, it is first of all necessary that the literature dealing with each of them should be read in a carefully discriminating spirit. Only after this, can a program of experimental work be drawn up with the object of testing the truth of the various statements which have been selected as possibly correct, and so finally arriving at a definite conclusion as to the methods best suited for the preparations required. All this necessitates such a volume of experimental and research work that it is obvious from the beginning that this is not an industry which lends itself to immediate adoption on a large scale. If it is to be successful in its final condition it must be allowed to grow as the knowledge and experience on which it must be based becomes available.

Practically every new substance prepared will entail a certain amount of research work, sometimes more, sometimes less; and in order that this work may be carried out expeditiously, it is necessary that a fully equipped and staffed research laboratory should be available. At the same time the demand for research work will fall off as the industry grows, and, though it will never cease, it will become spasmodic. When a demand arises for a number of preparations which have not hitherto been investigated, a large research staff will be fully occupied, and when the final reports on this work have been compiled, other work will have to be found for the research staff while the manufacturing laboratories are developing, on a requisite scale, the processes which have been evolved. Considerations such as these suggest that it would be preferable that the preparation of pure chemicals should be undertaken under conditions which would permit of use being made of one or other of the research laboratories at present in existence in connection with certain of our principal chemical industries, rather than that the new industry should be saddled with the expense of a large, and at times partially employed, research section.

The research work required in this connection need not be confined to a technical laboratory. There exists at present, throughout this and other countries, a strong feeling that everything possible should be done to encourage a closer association between the University on one hand and the industry on the other. With many industries a grave difficulty will be found to exist in the highly specialized knowledge and experience necessary to accomplish useful development work, while at the same time it is exceedingly undesirable that such close specialization should be introduced into the work of the University. In the case of pure chemicals, however, this difficulty does not exist. The work required is of a general and not a specialized nature and can readily be adapted to suit academic conditions. It possesses, moreover the further advantage that the University as well as the industry would benefit by association in this work, because it can be used both as an educational implement and as a means for the solution of technical problems, and would constitute a bond not only between the University and the industry of fine chemicals but between the University and many chemical industries. The work to be carried out under the auspices of the University would be embodied in reports describing in detail the best method which has been selected or devised for the preparation of particular chemical substances, having always in view the purity of the product and—a feature often overlooked by academical workers—the comparative economy of all the methods examined. Investigations of this nature, carried out by students under the guidance of their professor, would inculcate the necessity of making use of the literature of a subject and engender the ability to do so. They must be carried out under the best conditions as regards purity and yield of product and economy generally, and each stage then becomes essentially an accurate operation in quantitative analysis, in which not only the main reaction but also all secondary reactions must be closely followed with the view of promoting the former and discouraging the latter. The educational value of such work is obvious, and that there is room for its adoption, will be granted when it is considered that the analytical knowledge and skill possessed by the average University graduate, when he leaves his college, is at present far from satisfactory.

The importance of the purely scientific investigations which, in the past, have been the main object of University research, is not to be challenged, but if some of the rather pointless communications of doubtful scientific value were replaced by authoritative statements, on even the comparative value of various existing methods for the preparation of important compounds, the net result would be a material gain in useful knowledge. Now that our Universities are endeavoring to turn out graduates well equipped for technical work, investigations having some practical bearing will probably become more general than formerly, and this again should render subjects of the nature suggested more acceptable.

It would, of course, be necessary that those responsible for the industrial policy should be kept informed as to the progress of each investigation, and thereby a further advantage would be gained. Manufacturers would for the first time be brought into touch with students approaching the end of their University training and would be in a much better position ultimately to select the men who best suit the special requirements of their business than they are at present.

The ease with which the work can be selected so as to afford subjects of greater or less difficulty, should render it attractive both to Universities and to Technical Colleges, as enabling them to grade the work allotted to students in different stages of the training.

In addition to its educational value, coöperation between this industry and the Universities would doubtless result in the discovery of valuable commercial processes. The successful solution of the immediate problems arising directly out of the preparation of pure chemicals might not in themselves be of great financial value, because the total quantities involved are not large, and the amount of money to be invested in the industry is small in comparison with other commercial undertakings; but not the least of the charms of research work is the infinite possibility underlying it, and there are instances innumerable where a subsidiary and unlooked-for discovery has dwarfed into insignificance the successful solution of the original problem. In properly organized research on the preparation of pure chemicals, there is undoubtedly a great possibility of making discoveries of value in greater industries, and this should provide an incentive of no mean order to those undertaking the work. The question of the ownership of discoveries arising out of a study, by the University, of problems suggested by an industry, constitutes one of the preliminary questions upon which an agreement must be arrived at; but it should not be difficult to devise a working arrangement which would be satisfactory to both parties. Again, the value of many discoveries has been lost, or their useful application delayed owing to the inventor's want of knowledge as to where, and in what connection, his discovery can best be made use of; the industrial partner should provide a solution of this difficulty. The experts engaged in and responsible for the working of one industry cannot fulfil their functions properly without possessing a fairly intimate knowledge of the conditions and requirements of many other

*A paper read at a meeting of the Glasgow Section of the Society of Chemical Industry, and republished from the *Journal* of the Society.

industries connected, directly and indirectly, with that in which they are engaged. They should therefore be in a particularly favorable position to bring any new discovery before the people who are awaiting it most anxiously.

Now, as regards the products of this industry: all chemicals for laboratory use must possess a known and sufficient degree of purity, and in this connection the pamphlet on "Reagents for Analytical Purposes" issued by the Institute of Chemistry and the Society of Public Analysts last year, is a step in the right direction.

Nothing is more annoying to the chemist than to find, after he has spent much time and energy on an investigation, that his work is valueless and that his results must be discarded because one of the reagents used contained an unsuspected impurity. To quote a particular example, it was only after much work had been done and valuable time expended that a quantity of so-called "pure" phenanthrene, which had been purchased from a German firm, and accepted on its melting point and on the authority of a well-known label, was found to contain some seventy per cent of anthracene. That it gave the correct melting point of phenanthrene may or may not have been a coincidence!

To ensure satisfactory purity in the output in a manufacturing laboratory, such as is required for this work, it is necessary to institute a much more accurate control than suffices for ordinary manufacturing operations. As an indication of the refinements that are possible in this direction, it may be of interest to quote an instance from the records of the Nobel Research Laboratories at Ardeer. Some years ago certain discrepancies in analytical results were observed and, on a rigorous investigation, it was found that these could only be ascribed to variations in the degree of purity of the substances then used for the preparation of volumetric solutions. It was decided that this question should be set at rest once and for all by the selection of some one particular substance which could be obtained or prepared in a condition of great purity. This substance was to be termed the "Ultimate Standard," as far as these laboratories were concerned, and the purity of all other chemicals used was to be determined in relation to this standard. After careful study of the problem and consultation with the authorities of the National Physical Laboratory, who were of the greatest assistance, it was finally decided that metallic silver was the substance best suited for adoption as the ultimate standard. Silver cannot, however, be used directly for the standardization of acids or of oxidizing or reducing substances, and it was therefore decided to employ subsidiary standards for general purposes of control. The substances selected for these purposes and referred to as "Working Standards" were sodium carbonate and iodine. As it is only in the determination of atomic weights that the purity of a substance undergoes a rigorous examination, the work of preparing the ultimate and working standards and correlating their purity was carried out under conditions suitable for atomic weight determinations. The purest silver obtainable was first of all electrolyzed, then washed with sulphuric acid and water, and fused in lime crucibles in a current of hydrogen. The silver buttons were polished, washed with ammonia solution and water, dried, and kept in hermetically-sealed glass tubes.

The electrolytic method of purification removes all metals except gold and platinum, and the absence of these as well as of carbon is proved by the complete solubility of the silver in nitric acid. No trace of copper could be detected, although this metal was present in the original silver to the extent of 0.0007 per cent.

The iodine for the working standard was purified by mixing commercially pure resublimed iodine with ten per cent of potassium iodide and five per cent of calcium oxide and slowly resubliming the iodine in a special apparatus. The iodine collected was then resublimed twice in the same apparatus without addition. In the last sublimation a residue of 4 mgrms. was obtained from 350 grms. of iodine. The iodine from the last sublimation was powdered and kept over phosphorus pentoxide, for two months, and the dry iodine sealed up in small glass tubes each containing five or ten grms.

The preparation of pure sodium carbonate gave much trouble, and the final product was not satisfactory as regards absolute purity. One preparation only need be described. Seven hundred grms. of commercially pure anhydrous sodium carbonate was dissolved in 2.5 litres of distilled water at 25°. The solution was filtered into a narrow-necked flask and carbon dioxide passed into it. The carbon dioxide was previously washed with solutions of ammoniacal silver nitrate, dilute sulfuric acid, and sodium bicarbonate. After the air above the solution had been replaced, the flask was closed so that only the carbon dioxide passed into it which was required to take the place of that absorbed to form bicarbonate. When the process of saturation was complete at ordinary temperature, the solution was cooled to 0°, filtered by suction through hardened filter paper, and the precipitated bicarbonate washed with ice-cold water. The bicarbonate so obtained was then stirred in small quantities at a time into distilled water at 80°, until the solution was saturated. The solution was then allowed to cool until a small quantity of the salt had crystallized out. The mixture was then filtered quickly through a silver cone with fine holes. These first crystals contained all the mechanical impurities and were discarded. The solution was then cooled to 10°, the further crop of crystal collected in a funnel containing a silver cone and washed with small quantities of cold water. The mother liquor was concentrated three times in successive stages and each crop of crystals collected separately, the mother liquor from the last crop being evaporated to dryness. The purity of these fractions, after they had been heated in silver capsules at 250° to 300° C. until constant in weight, was determined in terms of the ultimate standard. The sodium carbonate content was as follows:

	Per cent
1st crop of crystals	99.906
2d crop of crystals	99.877
3d crop of crystals	99.896
4th crop of crystals	99.891
Residue from final mother liquor	99.857

The purest fraction, that of the first crystallization, was again recrystallized with the following results:

	Per cent
Crystals	99.904
Residue from mother liquor	99.902

From this it was concluded that the limit of purification possible by this means had been reached, although the final product still contained impurity to the extent of about 0.1 per cent.

In the determination of the silver-iodine ratio the iodine was reduced to hydriodic acid by hydrazine and precipitated by a known quantity of silver. Six determinations gave the following results for the ratio of iodine to 100 parts of the ultimate-standard silver: 117.691, 117.673, 117.657, 117.653, 117.650, 117.661. Mean 117.664. The following values for this ratio have been obtained by other operators: Merignac, 117.533; Stas, 117.534; Baxter, 117.656; calculated from international atomic weights, 117.650. This agreement was considered satisfactory, and for purposes of standardization in the Ardeer laboratories the ultimate standard is assumed to be of 100 per cent purity. On this basis the working standard iodine contains 99.99 per cent of iodine and the working standard sodium carbonate 99.93 per cent of the pure substance. A supply of these standards has been prepared sufficient to last for many years and the work can be accurately repeated at any time.

It is now proposed to utilize the organization which has grown up in the Ardeer Laboratories for the purpose of rendering available, to other laboratories, a supply of certain chemicals the quality of which can be absolutely relied upon.

As the principal object to be aimed at in these preparations is a high degree of purity, it is not intended that any of the operations shall be carried out on a manufacturing or large scale. The size of a working batch will be limited so that the accuracy of laboratory practice rather than of manufacture may be obtained, and in order that the products may be characterized by their homogeneity as well as freedom from impurity. A special manufacturing laboratory has been equipped for this work. It has been furnished with all the items of plant likely to be useful in work of this nature and can command the services of all the general factory workshops and tools.

Each final operation in this laboratory results in the preparation of a quantity of a finished material which, whatever its size may be, is given a lot number to identify it as a unit. After thorough blending, a sample of each lot is submitted to the analytical laboratory for examination, just as if the material had been purchased from an outside source. If the analytical report is satisfactory, the lot is passed for issue, and the essentials of that report are printed on the labels of the bottles containing it. These tested chemicals are issued to the usual chemical dealers for disposal. The simplest analytical report which could be issued would be a statement of the percentage of the particular substance which any preparation purports to be, leaving it to be assumed that the difference from 100 represents the impurity content. It is considered, however, that it is more useful to define the impurities which have been detected and leave the assumption to be made in the other direction. It is too much to hope that every impurity present in such a wide range of substances can be detected and its proportion determined, but the examination is made as thorough as possible and, where it is known that particular impurities interfere with the use of any reagent for a specific purpose, these of course receive special attention, both as regards elimination and detection.

Reproduction of Legs in a Salamander

We have under observation in the Reptile House of the New York Zoological Society, a very interesting specimen of the Austrian newt—a salamander-like creature that is reproducing fore and hind limbs on its left side. This creature was attacked by a larger specimen and both limbs were bitten off close to the body. The remarkable power of complete reproduction of the limbs among the tailed amphibians is well known, but the writer has never observed so interesting an example. From the rapidly closing wounds on the amphibian's body sprouted miniature appendages of perfect form except for shortened digits. In the early stage of reproduction, the minute limbs might have belonged to a creature of one-sixth the size. When the limbs were one-half the size of the normal ones on the opposite side, it was apparent that all muscular and tendon connections had been perfectly reproduced as the newt actually walked, though rolled to one side in order to bring the new limbs in close contact with the ground. The entire reconstructive period, to the full size of the new limbs, will cover a period of about three months.—N. Y. *Zoological Society Bulletin.*

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